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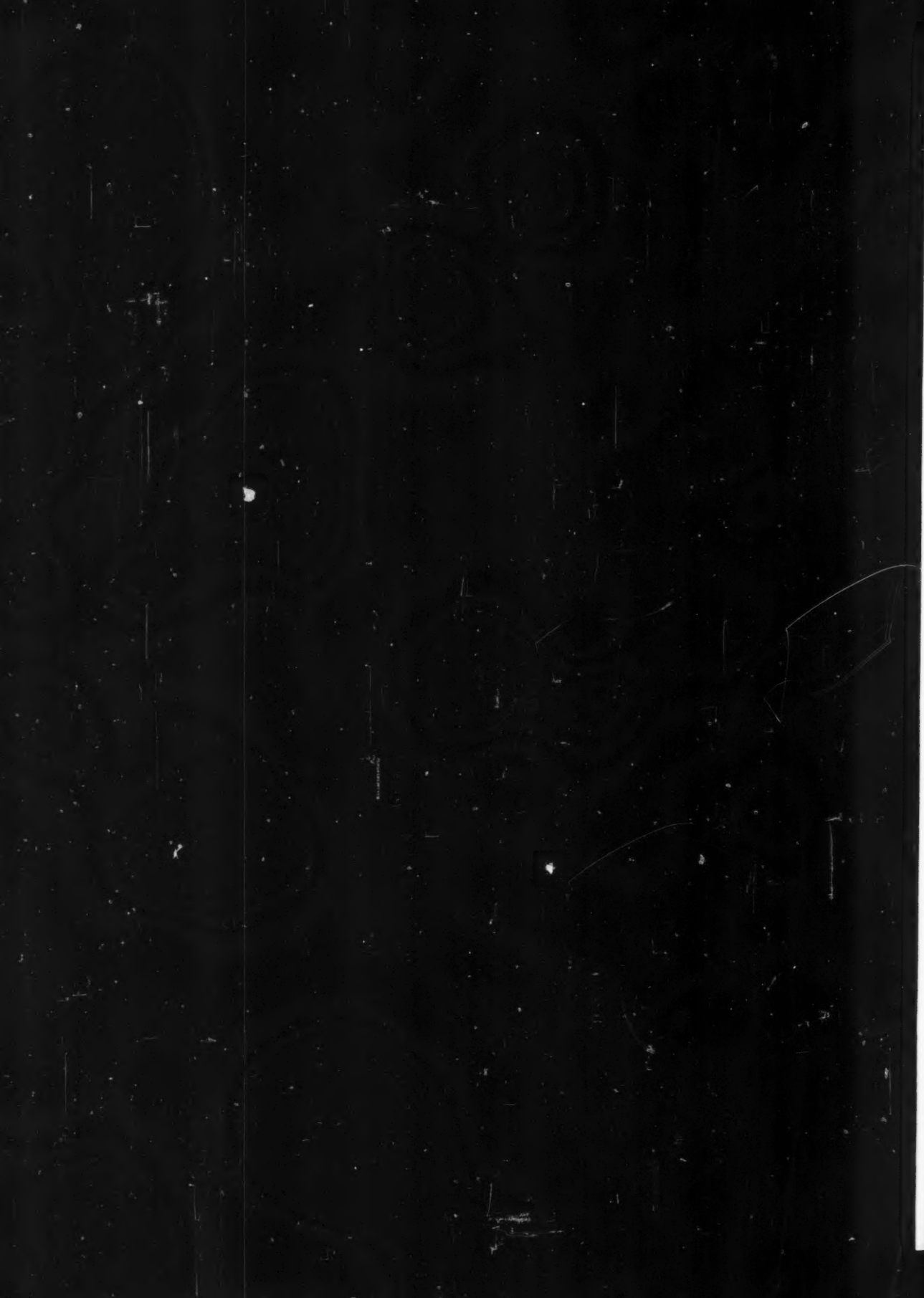

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ON A PROPOSAL TO USE THE EXTRAGALACTIC NEBULAE IN MEASURING THE PROPER MOTIONS OF STARS, AND IN EVALUATING THE PRECESSIONAL CONSTANT

WILLIAM HAMMOND WRIGHT

Director (ret.), Lick Observatory

(*Read April 20, 1946*)

FOREWORD

THIS paper, which is in the nature of an initial report upon a rather large astronomical undertaking, was read at the meeting of the American Philosophical Society held on April 20, 1946. Its publication has been delayed principally as a consequence of the writer's preoccupation with other matters. It seems appropriate to add that the project was initiated in the year 1934, through a generous provision of funds by the Carnegie Corporation of New York, and was carried on by the writer until his retirement from the Lick Observatory in 1944, and for a year thereafter through the courtesy of President Sproul of the University of California, and of Director Joseph H. Moore of the Lick Observatory. Dr. Moore was succeeded in office late in 1945 by Dr. Charles D. Shane, who decided to devote himself explicitly to the problem under discussion. The occasion seemed to be an appropriate one for a report upon the project, not only because of the personal consideration just mentioned, but also for the reason that the undertaking was at the time emerging from the developmental stage, and entering upon that of systematic observation. The paper is presented substantially as it was read. A few additional comments are provided in footnotes. As will be apparent from the beginning, the treatment is very general and was made as untechnical as practicable to suit the purposes of presentation to an audience of general cultural as well as of specifically scientific interests. It is hoped that this attempt at simplification has not contributed to ambiguity, nor obscured the scientific aspects of the subject.

Among the more important problems that invite the attention of the astronomer are those within the field of stellar dynamics; they relate to the structure of the Galaxy as a whole, and to the positions, masses, and velocities of its component stars. When one deals with positions and velocities, it is necessary first to establish a reference system—a system of coordinates if you will—with respect to which these positions and velocities can be measured. In our everyday experience we are, by force of circumstances, obliged to employ a variety of systems of reference for positions and motions: the surface of the earth, the walls of a room, the interior of a vehicle under acceleration, and so on. The astronomer is provided with no ready-made system that enables him to square himself precisely with the millions of stars, the planets and the rapidly moving comets with which he is confronted, but has managed to use planes and lines that are determined by the stars on the assumption that these apparently slow moving objects, at least, move at random, that is to say as much in one direction as another, and this means that, taken as a whole, the system which in their aggregate they compose may be regarded as without sensible rotation. Coordinate systems based on this assumption have served very well in measuring the comparatively rapid motions of the planets and comets in their travels across the heavens, but how far may they be trusted in measuring the extraordinarily small movements of the stars themselves?

That this is a valid question will be realized when we recall that the revolutions of the planets about the sun are expressible in quantities of the order of degrees per day; the angular motions of the stars, on the other hand, are as a rule less than one second of arc per century. The ratio between these two quantities is, in round numbers, 130 million to one. It is therefore comprehensible that a framework of reference which does very well for planetary motions might not be altogether adequate for the study of the extraordinarily minute motions of the stars themselves.

Consider, for example, one of the more important problems in the field of stellar dynamics: the measurement of the rotation of the galactic system of stars. The most obvious approach to

the solution of this problem is through the proper motions of the stars. But the coordinate system with respect to which these proper motions are measured has been determined on the hypothesis that the stars are moving at random, which is but a way of saying that the stellar system is without rotation. Recourse must then be had to special assumptions of one kind or another. Much ingenuity and labor have been directed toward the calculation of the galactic rotation, and there is no desire here to disparage in any way the results that have been achieved in that direction. Nevertheless it would seem desirable to approach the problem by a line of reasoning whose fundamental premise did not deny that the problem exists.[1]

The whole question of stellar proper motions would be much simpler were it not for the precession of the equinoxes, which, you will recall, imposes a conical motion of the earth's axis among the stars. This excursion of the axis has been likened to the motion of the axis of a dying top. The period of the motion is about 26,000 years. Now the unfortunate part of this is that the measurement of stellar positions is necessarily made with reference to the earth's axis and to the plane at right angles to it; that is to say the measurement is made with respect to a system of coordinates that is continually rotating with respect to the general system of the stars. As a consequence, when observations of position are made at two different times, or epochs as they are customarily called, in order to determine the proper motion of a star, it is necessary to allow for the motion of the earth's axis during the interval between the two observations, especially since the amount of this motion will generally be a thousand or more times as great as the proper motion of the star itself. The amount of the precessional motion is about 50″ per annum. The cause of it is well understood, and its amount would be calculable from the figure and content of the earth, the mass of the Moon, and other astronomical data were they known with sufficient accuracy for the purpose. Unfortunately we do not know the shape and structure of the earth, nor the Moon's mass, well enough to support a calculation of the precessional motion. It has therefore been necessary to determine the precession from the stars themselves. To a logician this would appear to be begging the question, and it is. The saving grace—if in logic there be one—is that there are a great many stars in the sky. If we choose a sufficiently large value of the precession, all of the stars may, on paper, be made to revolve in one direction; too small a value will cause them to go in the opposite direction. For an intermediate value some stars will revolve one way and some the other. What is usually done is to choose a value of the precessional motion which will make as many stars seem to move one way as the other.[2, 3]

The precession was discovered in the year 130 B.C. by the Greek astronomer Hipparchus, who provided the first estimate of its amount; it has been reevaluated many times since. The value now generally used is that derived by Simon Newcomb in 1898, and I venture to quote the opening paragraph of his discussion, as it closes with a note of prophecy which was soon to be fulfilled:[4]

> In his determinations of the elements of the four inner planets and the fundamental constants of astronomy the author was constrained to content himself with a provisional determination of the precessional constant, which he was afterwards led to fear might prove too small. One reason for yielding to the pressure of circumstances in this connection was that the constant of precession is of such a nature that a small error in its determination will not seriously affect our general conclusions as to the positions and motions of the stars and planets, such an error being eliminated through the proper motions of the stars and the mean motions of the planets. Indeed, the fact of this

[1] This comment relates to the approach based upon the evidence provided by stellar proper motions. The most convincing evidence that galactic rotation exists is of a different kind, and is supplied by the radial velocities of stars and of a few galactic nebulae. Rotation of such an aggregate as the galactic system quite certainly implies vortical motion within the system, and vortical motion in turn imposes differential velocities of a character that have actually been observed with the spectrograph. There are therefore two paths of approach to the problem of galactic rotation, neither of which may properly be neglected in a comprehensive study of the phenomenon.

[2] Mention should be made of an alternative method of establishing the stellar reference system through a dynamical study of the planetary motions, and without recourse to any assumption regarding the motions of the stars. The method was proposed by Newcomb, and has recently received much attention; it has been comprehensively and critically reviewed by Professor Dirk Brouwer (The Fundamental Properties of the Galactic System, *Ann. N. Y. Acad. Sci.* **42**: 133–149, 1941). Philosophically, a dynamical determination of an inertial system of coordinates is preferable to any other, if it can be satisfactorily established.

[3] August Kopff, Star Catalogues, Especially those of a Fundamental Character; George Darwin Lecture, *Mon. Not. Roy. Ast. Soc.* **96**: 716, 1936.

[4] *Astronomical Papers of the American Ephemeris*, **8**: 5, 1898.

elimination is one of the reasons why a satisfactory determination of the constant is difficult. There is, however, a class of researches which must come into prominence in the not distant future, to which the accurate determination of the precessional motion is a necessity. I refer to researches having for their object the determination of absolute and relative motions in the universe at large.

Some six years after the publication of Newcomb's paper, Kapteyn, from a study of proper motions in various parts of the sky, showed that the stellar motions are not, in fact, at random.[5] He found preferential directional trends in various parts of the heavens, which he assumed to be due to the occurrence of two star streams. These preferential trends disappear of course when summed over the whole sky because the precessional constant has been so chosen as to make them do so. There is no occasion here to go into the theory of star streaming as developed by Kapteyn and his followers, the point to be emphasized is that Kapteyn's observations showed that the stars are *not* moving at random. If they are arbitrarily constrained to the hypothesis of random motion for the whole sky they break out locally. Is it logical to admit preferential motion in large areas of the sky, and at the same time to accept random motion as the fundamental hypothesis of dynamical astronomy? In any event does it not seem desirable to inquire whether some system of reference might be established which could circumvent the necessity of making any assumption whatever regarding the proper motions of the stars themselves?

In these circumstances one naturally turns to the extragalactic nebulae which lie far beyond the limits of our stellar system, and can probably be counted upon to be uninfluenced by the motions of the stars that comprise that system. The matter has been the subject of some discussion in the literature, especially in recent years, on the part of astronomers in various parts of the world. This paper constitutes in effect an inquiry into the practicability of using the extragalactic nebulae as the material anchorages of a system of astronomical coordinates. A brief historical sketch of a current undertaking at the Lick Observatory directed toward that end may not be out of order.

I first entertained the idea of using the nebulae for the purpose just indicated in 1916, when it was coming to be realized that the spiral nebulae lie outside the Galaxy, but there was at that time no telescope at the Lick Observatory—nor in fact anywhere else—with which a research in that direction could be undertaken with hope of success. The observations would require a photographic telescope, or camera, of relatively wide field, so as to include a sufficient number of catalogue stars, as well as a lens of large linear and large angular aperture which would record small faint nebulae in the necessary abundance. Indeed it seemed questionable whether a lens of the requisite quality could actually be made. However, about the year 1920, Dr. Frank E. Ross designed a lens of comparatively large angular aperture which gives a very fine field, and thereafter the problem was much in mind. In 1934 these considerations were brought to the attention of the Carnegie Corporation of New York by Dr. R. G. Aitken, then Director of the Lick Observatory, and that organization very generously made provision for the construction of a 20-inch (50 cm) photographic telescope and a suitable observatory in which to house it. The lens was calculated by Dr. Ross. The construction of the telescope and the development of the project have been greatly hampered by the war, and the disturbed conditions that are following it, but one tube of the telescope has been completed, and satisfactory photographs are now being taken with it. At the present time the full equipment of auxiliary instruments for the measurement of the photographs has not been completed, but enough apparatus has become available to test the various observational procedures at the critical points, and more particularly to check the effects of departures from routine observational practice that have been imposed by the unusual conditions of the problem. These critical checks are vital to the success of the undertaking, and will be referred to later in this paper. For the moment I trust that I may be permitted to pass them with the comment that they seem quite satisfactory, and to proceed with a general account of the observations.

Stated in simplest terms the undertaking includes the following procedures:

A set of photographs is taken which covers, piece by piece, all of the sky that can advantageously be observed from Mount Hamilton. After a suitable lapse of time, which will be many years, another set will be taken for comparison with the first. The future observer will take up a pair of photographs of the same region of the sky, one from the first and one from the second series. These will each show several thousand stars, of which perhaps 15 or 20 will be catalogued stars

[5] *Rep. Brit. Assn.*, 257, 1905.

in whose motion we are especially interested. There will be, let us assume, 50 measurable nebulae on the photographs. In the interval between the taking of the plates the relative positions of the stars with respect to one another and to the nebulae will have changed. It is assumed that the nebulae, because of their enormous distances from our stellar system, will have remained substantially stationary on the celestial sphere. A correction is therefore made to all the measured positions—nebulae and stars alike—such as will cause the displacements of the nebulae to vanish. The resulting discrepancy in the position of each star, corrected where necessary for parallax, will then be regarded as its proper motion during the interval between the taking of the photographs. The question of precession of the equinoxes does not enter.

It will be observed that, while in the classical method of measuring proper motion observations are made all over the sky with the meridian circle, in the method here described each pair of photographs (except as indicated in the following paragraph) provides, independently of all else, the proper motions of all stars recorded upon it. It is anticipated that the proper motions of some 5,000 selected catalogue stars will be obtained in this way. These proper motions will, in general, not be exactly the same as the proper motions ascribed to the same stars in the catalogue. The discrepancies will be due in part to unavoidable errors of observation in both systems, and in part to the fact that the precessional constant used in constructing the catalogue does not conform to a system of coordinates tied into the nebulae. From a study of these inequalities it should be possible to correct the value of the precessional motion used in the catalogue by such an amount as would bring the two sets of proper motions into best agreement. By the use of this adjusted value of the precession the positions and proper motions of all the rest of the catalogue stars might be recalculated, and the coordinate system of the catalogue in question would, by such an operation, be moored to the extragalactic nebulae. However, it is not at all certain that this procedure will be exactly followed. Construction and repair of star catalogues are complex and laborious tasks, which call for the skill and industry of specialists in the field of meridian astrometry, and such matters are best left in their hands. It will doubtless be they who decide how the new proper-motion material can be most advantageously employed in the adjustment of the precession and in the revision of star catalogues.

It is necessary to call attention to the fact that not all of the photographs will be of use in the study of this particular aspect of our problem, that is to say in the direct determination of proper motions with respect to the extragalactic nebulae, because there is a part of the sky in which extragalactic nebulae are not found in sufficient number to meet the requirements of observation; that part of the sky is the great encompassing band of the Milky Way. The nebulae, although undoubtedly there, are curtained off by dark interstellar material, and are thereby either dimmed or completely hidden from view. The area within which extinction is fairly complete has been termed by Hubble "the zone of avoidance." The so-called "unobscured" areas of the sky, consisting of two broad segments of the celestial sphere on either side of the Milky Way, provide most of the nebulae. The boundaries of the zone of avoidance are both irregular and indefinite. Obscuration increases more or less gradually as the galactic plane is approached, and the border, as set by one observer, might very well differ from that drawn in by another. Our concern here is the establishment of a border within which nebulae will not be sufficiently plentiful to provide the necessary reference points on a star photograph taken with the Carnegie telescope under standard conditions, and this cannot be done until a survey of the critical area has been made with that instrument. For the purposes of the present discussion the borders of the zone of avoidance, as charted by Hubble, will tentatively be adopted.[6] In figure 1 the shaded area represents the zone to 30° south declination. The plat is based on Aitoff's projection of the sphere, which provides that equal areas of the sphere are represented by equal areas on the plat. Limiting one's attention to the part of the sphere above − 30°, it is clear that the areal ratio of the zone of avoidance to the part of the sky under consideration is the same as the ratio of the shaded area to the area of the chart north of − 30°. Measurement with a planimeter shows that ratio to be approximately 1 to 5. Assuming that nebulae are sufficiently numerous every-

[6] *The Realm of the Nebulae*, 62, Yale Univ. Press, 1936. Hubble's chart is in terms of galactic coordinates. In figure 1 of the present paper the shaded area represents the zone of avoidance as converted to the equatorial system. The transfer from the one system to the other has been made partly by hand sketch, and the chart here provided may differ in unimportant respects from the original.

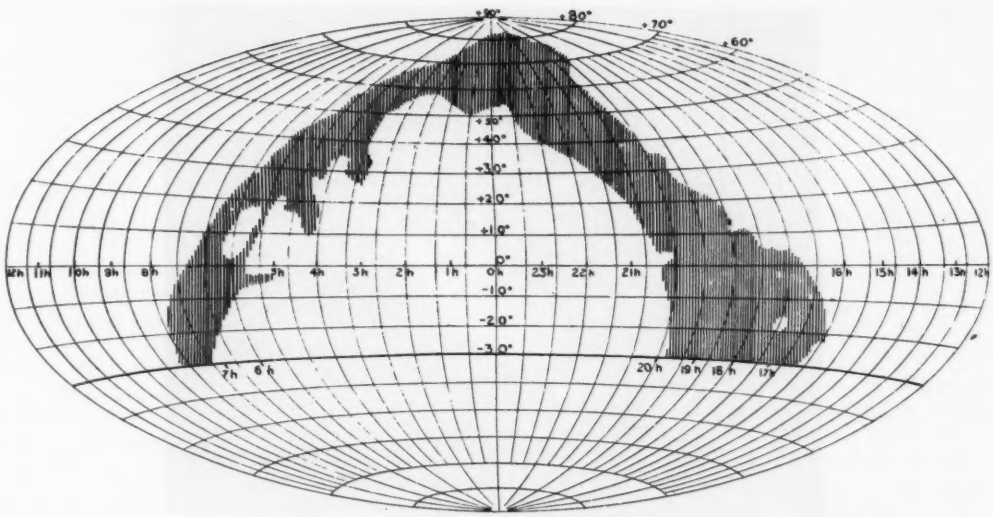


FIG. 1. Zone of avoidance by extragalactic nebulae (adapted from a chart by Hubble).

where outside the platted boundary of the zone of avoidance, it follows that proper motions should be measureable over about four fifths of the sky north of declination $-30°$. Probably three quarters would be a safer estimate; it is the value that will be assumed in this paper.

In the foregoing comments I have tried to indicate the need that has arisen for a system of astronomical coordinates that is free from the assumption of random motion of the stars, and have outlined in a very informal way the theoretical basis for such a system. It is now in order to inquire whether the observations and technics suggested for realizing the proposed system of coordinates are competent; whether the plan will work. That the difficulties are serious is indicated by the fact that the proposed system, or something like it, has not actually become established. I have already taken the liberty of referring to my early interest in the problem some thirty years ago, and to the lack of suitable equipment for undertaking it. So obvious a notion as that of using the extragalactic nebulae to check the proper motions of the stars must have occurred to many persons. My friend, Professor Lindblad, of the Stockholm Observatory has told me that he took some observations directed toward this end a few years ago, but found his equipment to be unsuited to the purpose. An attempt in Russia appears to have met with more success, but I am without definite information from there.

Speaking generally, the observational procedures employed in this investigation may be divided into two groups, as indicated in the accompanying tabulation:

Observational Procedures

(1) those which have generally been employed in astronomical photography, and may be regarded as routine.
(2) those which relate particularly to this problem, and are critical with respect to its validity.

The principal considerations in the second group are:

(*a*) the precision with which the nebulae can be measured.
(*b*) the number of nebulae available for measurement.
(*c*) the elimination of the magnitude equation in the measurement of bright stars with respect to faint nebulae.

The first of these groups contains procedures that have been generally employed in astronomical photography and may be regarded as routine; there is no occasion to discuss them here. In the second group are those considerations and procedures which relate particularly to this problem, and are critical with respect to its validity; that is to say failure in any one of them would invalidate the whole undertaking. It would be imprac-

FIG. 2. The Andromeda nebula as photographed with the Carnegie telescope. Original scale. Photo. by C. D. Shane.

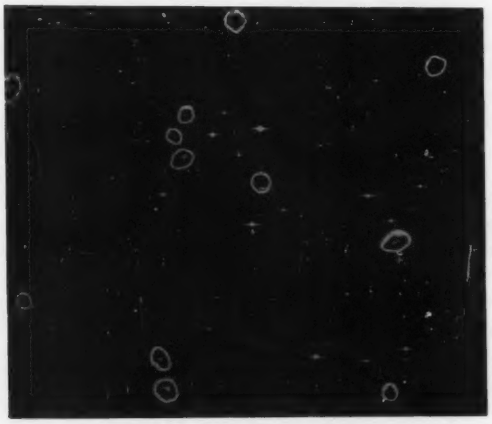


FIG. 3. Stars and nebulae; photographed with the Carnegie telescope. Encircled objects are nebulae. Position of center of (3): R.A. 12h 25.8m; Decl. +12° 23′ (1950). Original scale.

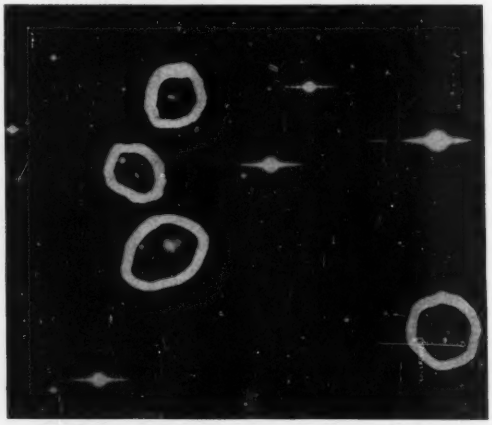


FIG. 4. Enlargement of central portion of figure 3.

ticable in a communication of this character to attempt an exhaustive discussion of these three items; on the other hand this report would be without value unless some account were provided of the exploratory surveys that have been undertaken to test them.

Items (*a*) and (*b*) are intimately related, and it will be convenient to consider them together.

Referring for a moment exclusively to item (*a*), the precision with which the position of a nebula can be measured, one's first thought, perhaps with one of the more spectacular nebulae in mind —for example, the Andromeda nebula, which is shown in figure 2—one's first thought is that because of the extensiveness of these objects, and their indefinite outlines, the accuracy of bisection [7]

[7] "Accuracy of bisection" is a term commonly employed in referring to the precision with which the wire in a

would not be very high. However, the Andromeda nebula is a comparatively near neighbor, presumably somewhat less than a million light years away. The farther off the nebulae lie, the smaller they appear, and the more numerous they become, until finally they record on a photograph as mere flecks of light, not much larger than images of stars. In figure 3, which is rendered in the same scale as figure 2, the small encircled objects are nebulae, though they are so small as hardly to be distinguishable as such. In order to make their nebular characteristics more readily appreciable, an enlargement of a part of figure 3 is provided in figure 4. It will be noted that, in these two illustrations, the brighter stars appear elongated. This is due to the employment of an objective grating, the effect of which is to flank the star images with a series of star-like dots. These dots are in reality extraordinarily short spectra symmetrically arranged with respect to the star image. They are used to indicate the position of a star when the image of the latter is too strong to permit its measurement. While these arrays of spectra, or secondary images, are carried by the fainter stars and by the nebulae, they are in most instances too faint to record on the photograph. Distant, and as a consequence seemingly small nebulae, such as are shown in figure 3, are especially desirable for measurement on three counts:

(1) they can be measured more accurately.
(2) they are farther away, so that such cross-motion as they possess will be less disturbing.
(3) they are more plentiful.

In any attempt to estimate the number of nebulae that one may expect to find in a given astronomical field, it is necessary to bear in mind that these objects are not uniformly distributed over the sky. Not only is there an apparent scarcity in the Milky Way, commonly ascribed to the absorption of light by interstellar matter of the Galaxy, as has already been mentioned, but there are restricted regions of the sky where nebulae are relatively plentiful. These are the so-called nebular clusters, and it may be remarked that figure 3 shows a part of such a cluster. Throughout most of the sky, however, the occurrence of nebulae is fairly uniform, and an equation by Hubble [8] enables the calculation of the number that may reasonably be expected to appear on one of the standard sized plates used in this work. The numbers of nebulae between successive integral values of the magnitude, beginning with the 11th, as calculated by Hubble's formula, are given in table 1, thus between magnitudes 13 and 14 one might

TABLE 1

NUMBERS OF NEBULAE, BETWEEN SUCCESSIVE MAGNITUDES, THAT MAY BE EXPECTED IN THE FIELD OF THE CARNEGIE TELESCOPE. SIZE OF PLATE: 43 CM SQUARE (42 SQUARE DEGREES)

Magnitude	11	12	13	14	15	16	17	18	19
No. of Nebulae	0.4	1.6	6.4	(24	93)	357	1268	4574	
					117				

hope to find six or seven nebulae. It is apparent that not much is to be expected from nebulae brighter than the 14th magnitude; there are not enough of them. It is, however, practicable to measure nebulae through a range of two magnitudes, and the double interval, 14 to 16 magnitude, promises 117 nebulae, of which perhaps half, or say 60, will prove measurable. It will be understood that these are but average figures; nevertheless counts on actual photographs provide a general confirmation of this estimate. It is regarded as desirable to have from 30 to 50 measurable nebulae on a plate, so that the estimated number of 60 allows some selection of the better defined objects, and is satisfactory.

The total number of (unobscured) nebulae between the 14th and 16th magnitudes, in the region of sky observable from Mount Hamilton, is estimated to be more than 40,000, and assuming half of these to be measurable, one may take as 20,000 the number available for observation. It is doubtful whether so many will be utilized.

While it will be necessary to wait some years to measure the proper motions of stars by the method here proposed, one need not wait at all to determine the precision with which the measurements, that is to say the micrometrical bisections of the images, will eventually be made. This can be done by measuring stars and nebulae on current photographs and noting the consistency of the observations, or rather the degree of their inconsistency. This inconsistency permits us to determine the so-called probable error of measurement, and it turns out that:

(1) the probable error of bisection of a nebula (in one coordinate) is..... $\pm 0''.125$
(2) the probable error of bisection of a star (in one coordinate) is ..... $\pm 0''.06$.

measuring microscope, or equivalent instrument, can be set across the middle of an object for the purpose of measuring its position.

[8] *The Realm of the Nebulae*, 71, Yale Univ. Press, 1936.

It may be assumed, for qualified comparison with these quantities, that

(3) the probable error of a meridian circle observation of a star (in declination) is ..... $\pm 0''.19$.

While the comparison might, at first glance, appear to indicate that the photographic measurements are more accurate than those made with the meridian circle, such a conclusion should not be drawn, at least on the basis of the present information. Both systems of measurement are affected by errors of bisection, and by instrumental and other errors as well. The photographic errors, (1) and (2), are merely those of bisection, while the meridian error is for the completed and reduced observation. Enlarged experience will permit a closer appraisal of the two methods of measurement; meanwhile it seems safe to assume that the position of a nebula can be measured photographically to the same general order of accuracy as that to which the position of a star is measured with the meridian circle. With this in mind we may revert to the estimate of 20,000 measurable nebulae in the sky. If but a fraction of that number of immobile objects can be measured on photographs, to the order of precision with which a star is measured with the meridian circle, it would appear that we have in them the means of stabilizing a system of celestial coordinates.

The foregoing considerations appear to resolve favorably the questions proposed in items (*a*) and (*b*). Attention may now be turned to item (*c*), the bridging of the gap of eight magnitudes between the brightness of the stars and the nebulae.

The general problem in hand involves the measurement of a group of bright stars, ranging in magnitude from 6.2 to 8.2, with respect to a group of nebulae which are vastly fainter, ranging in fact from 14.2 to 16.2 magnitude. These are only approximate figures, but they indicate the character of the undertaking. There is, except in the matter of degree, nothing new in the proposed operation. It has long been recognized that a source of error, termed the "magnitude equation," enters into the measurement of bright stars with respect to faint ones, and various methods have been devised to allow for it. One of the most convenient and successful of them employs an objective diffraction grating, and that device has been adopted in the present investigation, as has already been indicated. The unusual feature here, however, is the greatness of the gap in brightness as between the nebulae and the stars. This gap is of the order of eight magnitudes, which, expressed as a luminosity ratio, is 1 to 1,600. In other words, the stars to be used are, on the average, 1,600 times as bright as the nebulae. This is altogether too wide a gap to be bridged by methods in use, and has called for the development of a special technic. The new procedure involves the introduction of a short supplementary exposure to the star field, which permits the comparison to be made in two steps. It is convenient to explain the routine with the aid of the diagrammatic sketch shown in figure 5. For simplicity it is assumed that the catalogue stars are all of the 7th magnitude, the nebulae of the 15th, and that the grating step is 4 magnitudes. It is also assumed that all of the objects lie on a single line determined by the diffraction pattern of the stars. The standard exposure for the nebulae is 2 hours. One hour from the beginning, which will be when the telescope is on the meridian, the exposure is interrupted, and the plate is moved approximately 1 mm in the direction perpendicular to the line of the diffraction patterns. An exposure of approximately one minute is then given, the plate is returned to its original position, and the principal exposure is completed. The length of the short,

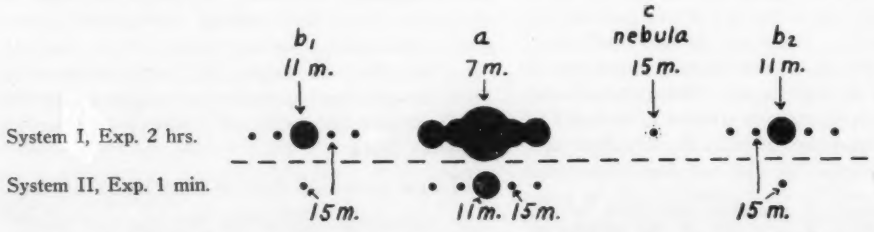


FIG. 5. Diagram illustrating the device employed in bridging the eight magnitude gap between catalogue stars and nebulae.

or supplementary exposure is determined by trial as that which will give a density of the direct image equal to the density of the first order spectrum in the principal exposure. We term the principal exposure System I and the supplementary one System II.

In the figure, "*a*" is a catalogue star of the 7th magnitude. In System I the central image spreads over the first and second order spectra, but in System II the first, or in any event the second order spectra of this star are measurable.[9] (b_1) and (b_2) are 11th magnitude stars, which are very plentiful and can be found practically anywhere on the plate. A sufficient number of them, properly distributed through the field, is selected for measurement. In this paper these will be called control stars. The spectra of these stars are measurable in System I, and the central image is measurable in System II. The purpose of these measurements is to establish the corrections that are necessary to enable one to pass, in the reduction of the observations, from System I to System II or *vice versa*. If the displacement of the plate has been exactly parallel to itself, the corrections will be the same for all parts of the field; otherwise they will vary a little from one part to another. However that may be, experience has shown that it will not be difficult to establish a relationship between the two systems. It will be borne in mind that the nebulae are measured in System I, using their direct images, while the catalogue stars are measured in System II, using their first or second order spectra. Conversion from one system to the other is effected, as just indicated, by simple additive corrections to the measurements of the stars or the nebulae, as the case may be. It will be observed that, in bridging the gap of 8 magnitudes, images of the same density are used throughout; no magnitude equation is involved. There are aspects of these operations that merit further experimental test, but there seems to be no reason to anticipate serious difficulty in developing a satisfactory procedure.[10]

In the foregoing brief account of the proposed method for circumventing the magnitude error, the range of two magnitudes within each of the two groups, stellar and nebular, has been disregarded. No difficulty is anticipated in deriving, by routine methods now in general practice, whatever correction of this character may be found necessary.

The necessity for using intermediate, or control stars, in the manner just described is of course to be regretted, as the employment of the two stages into which the procedure is thus divided will add to the labor of measurement and reduction, and will diminish, in a degree, the accuracy of the end results. On the other hand the measurements of these 11th magnitude stars will provide the necessary data for calculating their positions and proper motions. These proper motions should be somewhat more dependable than those obtained for the catalogue stars, for the reason that there is but one measurement between the control stars and the nebulae. Thus there should eventually become available a group of stars of approximately the 11th magnitude, quite uniformly distributed over the sky (barring the zone of avoidance) at the rate of perhaps one per square degree, with especially well determined proper motions. The reduction of these measurements could be effected when the data were desired.

In what has preceded it has been assumed that the present undertaking aims only at the measurement of the proper motions of the stars, and that the precession is to be evaluated through a comparison of the results so derived with similar data obtained with the meridian circle and recorded in the catalogues. In reality there appears to be a substantial prospect that the precession itself can be obtained from photographs taken in the

[9] When the position of a star is measured by means of its spectra, equidistant spectra on each side are measured and the mean is taken as representing the position of the star.

[10] A criticism of this plan of measurement has been advanced to the effect that the correspondence between the principal exposure and the short supplementary exposure might be vitiated through the occurrence, during the shorter exposure, of extraordinary disturbances in "guiding" or in "seeing." In so far as faulty guiding is concerned, the effect should be felt equally all over the plate, and, since the measured images are all of substantially the same density, it is not clear why they would not all be affected alike, with zero influence on the result of the comparison. In respect to "bad seeing," if that were of such a nature that the field was not distorted, the case would be the same as for faulty guiding. If, on the other hand, local displacements in the field were occasioned by the seeing, that fact would be evident from the measures of the 11th magnitude control stars (b_1, b_2, fig. 5) which are measured in both exposures. If these control stars are sufficiently numerous, and are favorably distributed over the plate, they should provide the corrections necessary to compensate for the field distortions caused by abnormalities of seeing during the short exposure. It would be especially advantageous to have control stars close to the catalogue stars. The criticism is of value in emphasizing the desirability of careful selection of the control stars.

vicinity of the pole. The procedure would be to establish the position of the pole from time to time by a modified method of circumpolar trails, and to measure its march, as so determined, with respect to the field nebulae. This march is but the reflexion of the precession. The routine would probably be to take the circumpolar photographs at frequent intervals, and on small plates, using comparatively close and faint circumpolar stars as points of reference for the position of the pole. These stars would be correlated with the nebulae with the aid of photographs on full-size plates taken at longer intervals of time. A question arises as to whether the telescope mounting is sufficiently stable to permit the location of the pole, with requisite precision, by circumpolar photographs. The instrument itself is, of course, at rest during this observation, so that only the consideration of static stability is involved. If it should in fact prove unequal to the occasion, other means of making the circumpolar observations might very well be devised. Inasmuch as the telescope would be immobile, the mechanical requirements of the instrument should be simple.[11]

Two not too favorable circumstances present themselves in a consideration of the immediately preceding suggestion. One is the relative scarcity of nebulae in the vicinity of the pole which, with a galactic latitude of about 27°, lies close to the zone of avoidance; undoubtedly there is some obscuration there. A count of nebulae on a two-hour exposure to the polar field reveals 37 objects, of which 30 are regarded as measurable; experience will show whether they are sufficient. The second element of weakness is that the determination would be made on the basis of a single star field, while the other method avails itself of many fields taken over a considerable area of the sky. For this reason the interval of time required to obtain photographs for the polar-trail method would be much greater than for the other. On the other hand, the polar-trail method would provide a completely independent value of the precession, and that is a substantial consideration in its favor.

Basic to this whole problem are of course the distances of the extragalactic nebulae, and the speeds of their cross-motions, since these determine the proper motions of the nebulae, which are in this paper regarded as negligible. Is the ratio of cross-motion to distance sufficiently small to be neglected in establishing a fundamental system of celestial coordinates? The writer is not aware that differential proper motions as among the nebulae have been measured. It is conceivable, though it seems unlikely, that they will emerge from the present or some similar investigation. If they are ever detected they will bear upon our present hypothesis, but for the time being we must be content with existing knowledge. A possible approach to the evaluation of the nebular proper motions might be through a study of the mass of the universe, and its effect on the motions of the nebulae, but the manner of calculating such an effect is not at once apparent. If the interpretation of the "red shift" in terms of radial velocity is correct it seems clear that the methods of Newtonian mechanics, which are employed in the handling of a similar problem within the galactic system, would be of little help, for an "expanding universe" is not simply interpretable in terms of Newtonian gravitation.

The best substantiated information we have respecting the motions of the nebulae is that their radial velocities are approximately proportional to their distances from us. At great distances these radial velocities become so considerable as to suggest that they provide the largest component of a nebula's speed. On the basis of that assumption one may calculate a maximum value of the proper motion by turning the motion of a nebula around at right angles and letting the body travel at its previous radial speed of recession. This should provide a hypothetical maximum proper motion of the nebula. Hubble's equation in expression of the radial velocity as a function of the nebular distance is:[12]

$$v = 530d,$$

where v, the radial velocity, is expressed in km per sec and d, the distance, in million parsecs. By laying this off in a direction perpendicular to the

[11] At the time this paper was prepared (in 1946) I was unaware of the very interesting work of Dr. Dirk Brouwer, at Yale, directed toward the study of the nutation. In his investigation he locates the celestial pole by stellar trails, using the Loomis telescope of the Yale Observatory. I have had the privilege of seeing one of Dr. Brouwer's excellent photographs, and it appears to substantiate the view that it will be practicable to study the motion of the pole in this way. His purpose is understood to be principally the investigation of the nutation, while at the Lick Observatory it will be necessary to have both nutation and precession under study, and there will be, in addition, the reference of positions to the extragalactic nebulae. There seems here to be the possibility of some cooperative effort, as the particular stars used as circumpolars at Yale could obviously be referred to the nebulae through the instrumentality of the Lick Observatory plates.

[12] *Ibid.*, 170.

line of sight, we calculate the hypothetical maximum nebular proper motion (per year) as:

$$\text{p.m.}_N \leqq 0''.000106.$$

This is the value for a single nebula. If we may assume that the cross-motions are at random, it is obvious that, in their mean, the motions of the nebulae will be of no significance whatever in relation to the present problem. On the other hand, if the motions were systematic the effect would remain negligible for a very great length of time. The quantity is of the order of one per cent of the probable error of our present knowledge of the precessional constant.

A question frequently asked regarding this project is how long it will take to complete it. That is a matter that will be better determined as the work progresses, and such remarks on this aspect of the undertaking as are offered here are to be regarded as very tentative in character. When the work was begun it was vaguely estimated that the two sets of photographs forming the basis of the investigation would require to be separated in time by about one hundred years, but it now appears that much information can be derived from a smaller separation. It will be recalled that, in reality, two problems have been set, and a third is being considered:

(1) the measurement of stellar proper motions.
(2) the adjustment of the precession to these proper motions through the intermediacy of meridian circle observations and star catalogues.
(3) the independent determination of the precession from the march of the celestial pole, as determined by circumpolar trails.

There has been little opportunity to experiment with item (3), and a prediction of the time required for its completion has not been attempted.

Before attempting to form an opinion of the length of time required by either of the first two items, it is of course necessary to specify the precision to be sought in the evaluation of the proper motions of the stars and in the determination of the precession. If we take 0".001 per year as the probable error to be expected for the individual proper motions of the stars, it seems likely, on the basis of tests that have already been referred to, that a separation of about one hundred years will be required. On the other hand, if the same degree of precision were aimed at in the precession, a much shorter interval would suffice, for the reason that many stars enter into the determination of the precession, and advantage is had of the law of averages; specifically, one does not have to know the proper motions of the stars to 0".001 in order to attain that accuracy in the precession. At the present writing it seems as though a probable error of 0".001 in the precession might be had with an interval of ten years between photographs, though in view of the amount of labor involved in measurement and reduction it is doubtful whether an exhaustive calculation on so short a base would be regarded as desirable, where more competent data could be counted upon to come later. However, the necessary plates will doubtless be on hand in case it is desired to use them, for it will probably be found advantageous, on a number of counts, to repeat the photographs after an interval of five or ten years.

SUMMARY

As stated in the text, this paper constitutes an inquiry into the practicability of using the extragalactic nebulae as the material anchorages of a system of astronomical coordinates; it relates especially to an attempt currently being made at the Lick Observatory to accomplish that purpose.

The need of some sort of stabilization of the astronomical coordinate system has been felt in increasing measure for a long time. It had its beginnings in the detection by Halley in 1718, and later by others, of the proper motions of a number of stars. Before Halley's time the stars were regarded as forming an invariable pattern in the sky; differential motion among them was not known. They therefore provided the basis for a satisfactory framework to which the positions and motions of the heavenly bodies might, without ambiguity, be referred. With the discovery of proper motions it came eventually to be realized that all the stars are moving about in the sky; this unsettled the old motion of an invariable firmament and a unique reference system, since, from a kinematical point of view, and without regard to dynamical considerations, any combination of two stars (together with the observer) is competent to determine a coordinate system. Except for accidental coincidences there are vastly more such possible systems than there are stars. In order to approximate to a dynamical solution it was assumed that the stars are moving at random, and, on the basis of that hypothesis, it was possible to establish a sort of mean coordinate system which in fact satisfied the requirements of astronomy for a long time, and is still in use.

However, in 1904, Kapteyn showed that many stars have proper motions that are incompatible with the premise of random distribution. This disturbing revelation, which strains the basic assumption upon which the coordinate system rests, came at a time when the demands of astronomy for a stable system of reference were increasing. During the years that have followed, the concept of an extragalactic system of nebulae has developed. There are substantial reasons for regarding these nebulae as very far away, and not subject to appreciable influence by dynamical conditions within the Galaxy. They therefore hold the promise of a stable system of coordinates, unaffected by dynamical conditions within the Galaxy, provided the transition from the stellar to the nebular system can be effected. The procedure by which it is sought to effect this transition is to measure the proper motions of a sufficient number of stars with respect to the extragalactic nebulae. The stars and nebulae are on the same photographic plates, and the measurements have the advantage of being differential. The independent determination of these proper motions removes the ambiguity originally introduced by the proper motions of the stars, and imposes upon the stars observed, and upon others associated with them in a comprehensive catalogue, the same reference standards as were used in measuring the new proper motions, namely the extragalactic nebulae.

The instruments and observational procedures employed in the work at the Lick Observatory are described as fully as seems necessary for the purposes of this paper. It appears that there are many thousands of nebulae distributed over approximately three quarters of the sky, whose positions can be measured photographically to the same order of precision as the places of stars are measured with the meridian circle. These nebulae are deemed sufficient to provide reference points for as many stellar proper motions as may be required. For reasons given in the text, it is anticipated that the proper motion stars will be limited to magnitudes 6.2 to 8.2, though material will be available for the calculation of the proper motions of stars between magnitudes 10.2 and 12.2. The conclusion is drawn that the project is feasible, and will, with the collaboration of meridian astronomers, result in the establishment of a practicable system of coordinates positioned by the extragalactic nebulae.

In conclusion it is a pleasure to acknowledge the assistance of Dr. Julia Vinter Hansen, of the Copenhagen Observatory, rendered while she was a guest of the Lick Observatory during the years 1940–45. Dr. Vinter Hansen collaborated in the preliminary observations, and performed various calculations. Her own work with the telescope, in which she measured the positions of comets and asteroids, served to demonstrate the excellent mensural qualities of the new instrument.

DISTRIBUTION OF EXTRAGALACTIC NEBULAE IN THREE SELECTED AREAS

C. D. SHANE AND C. A. WIRTANEN

Lick Observatory, University of California

(*Read April 22, 1949*)

THE study of the distribution in space of the extragalactic nebulae may be approached from two aspects: one, their distribution in depth, or two, their distribution over the surface of the sky.

In the first case, one may investigate their general increase in number when they are counted to successively fainter limits. Since the apparent brightness of a nebula depends mainly on its distance, this type of investigation is essentially an exploration of space in depth. Its main purpose is to relate the real or apparent space density of nebulae to distance from our own stellar system.

To an overwhelming degree, our present knowledge in this field is based on the work of Hubble.[1] The apparent distribution of the nebulae, based on magnitudes uncorrected for the red shift, indicates a density that decreases with increasing distance from our stellar system. If the magnitudes are corrected for the red shift on the assumption that the shift is due to velocity of recession, then there is an apparent increase in the density of the nebulae with increasing distance. This implies a strong space curvature that can scarcely be reconciled with our present knowledge of the density of matter. Correction for the red shift on the assumption that it is due not to velocity, but rather to some other and unknown cause, yields a density that does not vary appreciably with distance. The results are uncertain since the sample of explored space is severely restricted, and there is some question concerning the accuracy of the limiting magnitudes of the nebulae. The greater penetrating power of the 200-inch telescope, and the efficiency of the 48-inch Schmidt at Mt. Palomar for nebular counts, are expected to go far toward removing the uncertainties that surround this problem.

In the second case, the approach to the problem of nebular distribution is the determination of possible variations of space density that are not related to distance from our stellar system. The obvious method is to count nebulae over the sky to the same faint limiting magnitude. Significant variations in the counts can then be interpreted in terms of space density variations. It is well known that the nebulae have a strong tendency to occur in groups and clusters. The problem is to discover and study any further structural features that may exist in the assemblage of nebulae. Shapley's counts [2] of nebulae in the south galactic polar cap indicate a strong gradient in density over an angular distance of about 50° of the sky which corresponds to 10^8 light years in linear measure. He has also called attention to the greater numbers of nebulae per square degree in the northern galactic hemisphere as compared with the southern. Hubble, on the other hand, concluded from his numerous small-sample counts which extended to greater distances that there is no distinct evidence of systematic variations in density apart from local clustering.

The problem is complicated by the existence of absorbing matter in our own stellar system. This material is irregularly distributed near the galactic equator, although in some regions it extends into fairly high galactic latitudes. The effect is to decrease the nebular counts in an irregular manner over much of the sky. The region near the Milky Way in which the nebulae are almost completely obscured has been delineated by Hubble, who has termed it the "zone of avoidance." While the absorbing matter interferes with the interpretation of nebular counts in terms of the nebular distribution, the counts may in turn be used to study the distribution of the galactic absorbing matter. Since the two problems are closely related, they may be considered together.

In accordance with the second method of approach, this paper presents some very preliminary results of such a combined study of the galactic absorption and the apparent distribution of the nebulae. Plates taken with the 20-inch Carnegie Astrograph of the Lick Observatory were used

[1] Hubble, Edwin, The distribution of extragalactic nebulae, *Astrophys. Jour.* **79**: 8–76, 1934. Mt. Wilson Contr. No. 485.

[2] Shapley, Harlow, A metagalactic density gradient, *Proc. Natl. Acad. Sci.* **24**: 282–287, 1938.

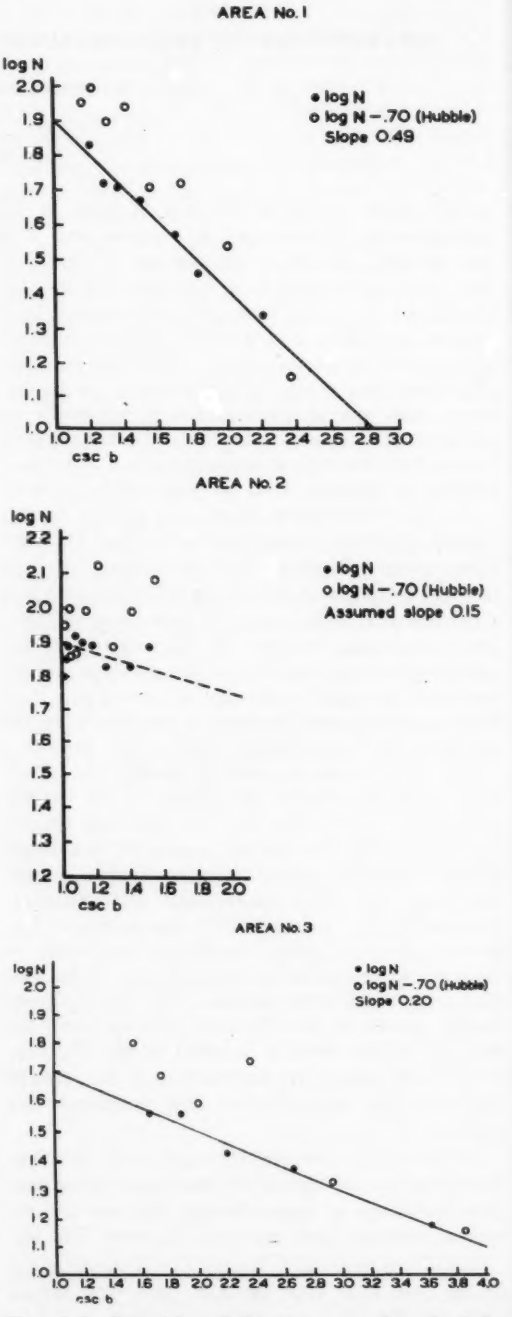
for this study. These plates are part of a series being taken on a program planned by Dr. W. H. Wright [3] prior to his retirement. It is proposed to compare this series with a second series to be taken several decades later, and thereby to determine the proper motions in our stellar system by use of the extragalactic nebulae as standards. These plates are well adapted to counting nebulae on a uniform basis. Each exposure of two hours duration is centered accurately on the meridian. The plates are seventeen inches or six degrees square and record stars to the nineteenth magnitude. They are centered five degrees apart so that in general a border one degree wide is duplicated on adjacent plates. Overlapping counts in these borders serve to determine correction factors for varying emulsion sensitivity and for systematic counting errors from plate to plate. The counts are corrected for atmospheric extinction and for distance from plate center. Thus it is believed that a fairly homogeneous system has been secured.

The nebulae are counted and recorded by 10-minute squares, or by 36 counts to each square degree. All objects are included which experience indicates are at least 90 per cent certain to be nebulae. Thus far the observations and counts have been completed in three irregularly shaped areas having the following approximate galactic coordinates:

Area 1. Centered about $l = 330°$ and $b = +40°$, covering 975 square degrees.
Area 2. Centered about $l = 270°$ and $b = +60°$, covering 925 square degrees.
Area 3. Centered about $l = 45°$ and $b = -25°$, covering 1,000 square degrees.

Let us first consider the relation between average numbers of nebulae per square degree and galactic latitude to see whether the counts show the effect of absorption in our stellar system. In figures 1 to 3 we have plotted against csc b for various mean galactic latitudes, the logarithm of the average number of nebulae per square degree. The logarithms of our counts are represented by dots. The logarithms of Hubble's counts from his systematic survey, diminished by 0.70 for convenience in plotting, are indicated by circles. It is easily shown that if the nebular distribution were random, and if the absorbing material in

[3] Wright, W. H., On a proposal to use the extragalactic nebulae in measuring the proper motions of stars, and in evaluating the precessional constant, *Proc. Amer. Philos. Soc.* **94**: 1–12, 1950.



FIGS. 1–3. Logarithm of the average number of nebulae per square degree plotted against the cosecant of the galactic latitude.

our system were composed of uniform layers parallel to the galactic plane, the plotted points in the figures should fall on straight lines with the same slope. In figures 1 and 3 the points do fall approximately on straight lines, but the slopes are widely different. This difference in slope is due to the known fact that the absorbing matter is irregularly distributed. No very reasonable departure from a random nebular distribution could account for such large differences in slope. The approximately linear placement of the points may be due in part to a chance arrangement of the absorbing matter, and in part to a certain insensitivity to irregularities in its distribution.

In figure 1 the slope is 0.49, while in figure 3 it is 0.20. From nearly all his survey samples, Hubble derived an average slope of 0.15. The large slope in figure 1 is to be expected from the fact that in this region of the sky, the zone of avoidance has a bulge extending into rather high latitudes. Hubble's observations in these two areas yield somewhat larger slopes than do ours. Area 2, shown in figure 2, is in such a high latitude that no significant solution for the slope can be made.

The data portrayed in figures 1 to 3 yield the difference in limiting magnitude between Hubble's counts and ours. In Areas 1, 2, and 3 Hubble counted respectively 7.1, 5.8, and 6.8 times as many nebulae per square degree as we did. The mean ratio is 6.6. According to Hubble's determination,[4] a factor of 3.2 in the number of nebulae counted corresponds to a step of 1 magnitude. Thus the difference in limiting magnitudes is log 6.6/log 3.2 = 1.6. If we apply this difference to Hubble's value of 20.0, we obtain 18.4 for the limiting magnitude of our counts. This is about 0.6 of a magnitude brighter than the faintest stars discernible on the 20-inch plates.

In order to exhibit the apparent distribution of the nebulae graphically, equal density contours were plotted for the three areas. When the numbers of nebulae per square degree are plotted, they run so irregularly that it is not feasible to

[4] Hubble, Edwin, The effects of red shifts on the distribution of nebulae, *Astrophys. Jour.* **84**: 517–554, 1936. Mt. Wilson Contr. No. 557.

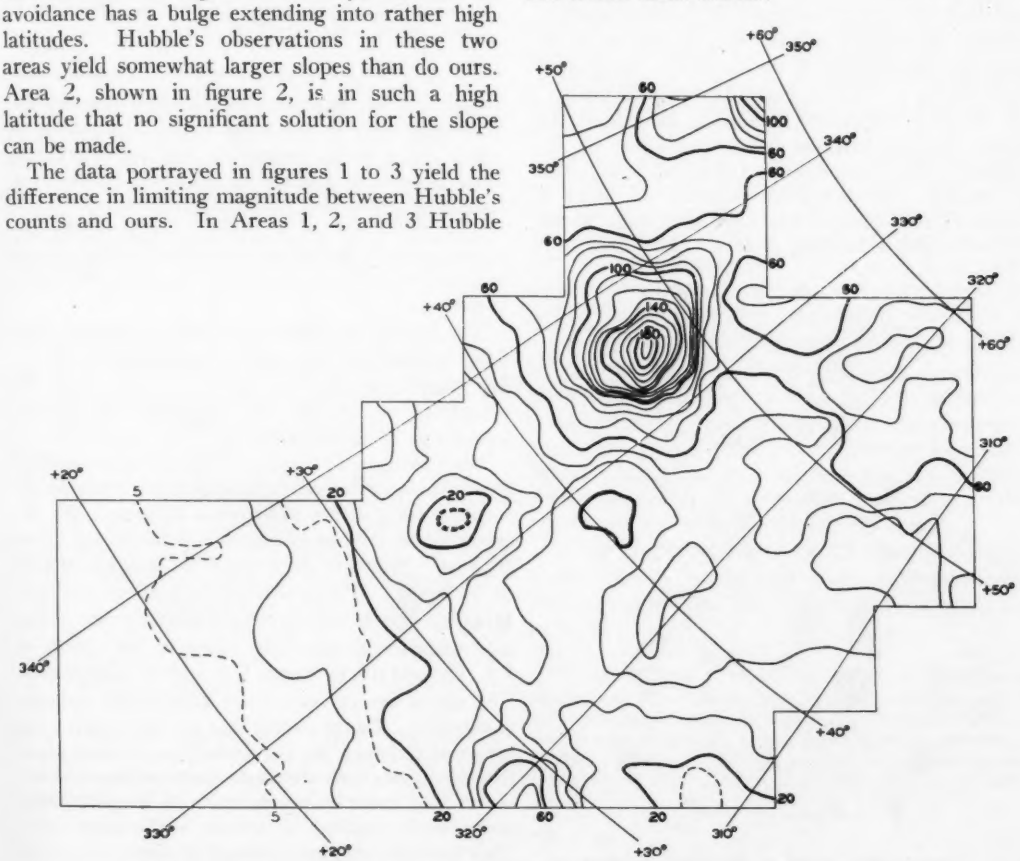


FIG. 4. Contour map showing smoothed numbers of nebulae per square degree for Area 1.

draw contours. Accordingly, the numbers were smoothed by averaging with the value for each square degree the values for the surrounding 8 square degrees. Thus in general each value is a mean of the counts for 9 square degrees. This smoothing process results in contours that are not too complicated to draw.

The density contour maps are reproduced in figures 4 to 6. Contours were drawn for intervals of 10 nebulae per square degree. The contours for 20, 60, 100, etc., are numbered and are drawn with heavy lines. In addition, broken lines represent the 5 and 15 contours. It should be noted that, owing to the smoothing process, the relief as portrayed is greatly suppressed. Areas of high density are reduced and those of low density are increased. The number 5 contours in figures 4 and 6 coincide rather closely with Hubble's boundary for the zone of avoidance. One gains the impression that the pattern in figure 6 is dominated by galactic absorption, while that for figure 5 reflects the distribution of the nebulae. In figure 4 both effects are prominent. The typical clustering of the extragalactic nebulae is evident in the higher galactic latitudes, while the effect of absorption becomes more apparent toward the galactic equator. Of particular interest in figure 4 (Area 1) is a zone of low nebular density about 10° wide and extending from $l = 340°$ and $b = + 37°$ to $l = 310°$ and $b = + 33°$. This is a previously known region of galactic absorbing matter. Within it there is one area, 15 by 20 minutes, in which not a single nebula or star appears. The most prominent feature in figure 6 is the long tongue of absorption extending from $l = 37°$, $b = - 19°$ to $l = 41°$, $b = - 28°$. Some of the enclosed areas of low density, indicated in the figures by hatched contours, probably signify absorbing clouds.

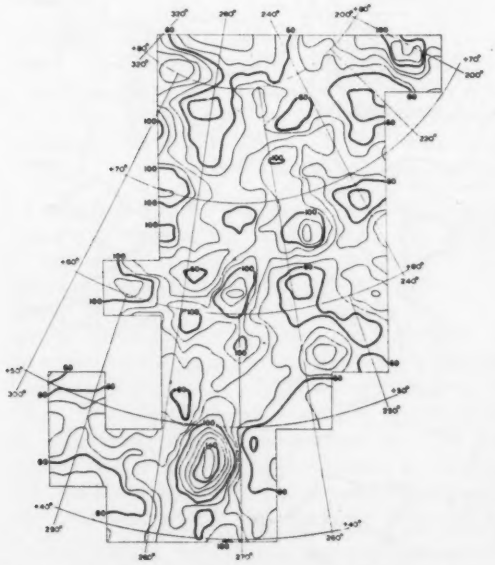


FIG. 5. Contour map showing smoothed numbers of nebulae per square degree for Area 2.

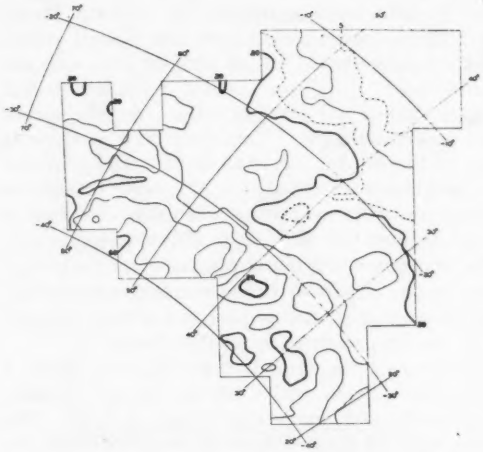


FIG. 6. Contour map showing smoothed numbers of nebulae per square degree for Area 3.

The limited amount of material presented here does not warrant any general conclusions regarding galactic absorption, but it indicates that the completed survey will add substantially to our knowledge of this subject.

We turn now to the distribution of the nebulae. We cannot as yet make a sufficiently reliable allowance for galactic absorption to permit the intercomparison of numbers of nebulae in the three regions. There is, however, one suggestive fact to be noted. It was previously pointed out that Hubble's counts, which extend nearly twice as far into space as do ours, bear ratios to our counts of 7.1, 5.8, and 6.8 in Areas 1, 2 and 3, respectively. The ratios are of course not affected by galactic absorption. To the extent that we may regard the ratios as constant, we conclude that in three separate directions the extension of a cone from the observer into space by nearly twice its length always increases the number of nebulae in the same ratio. This strongly suggests, though it does not prove, a constant large-scale density of nebulae.

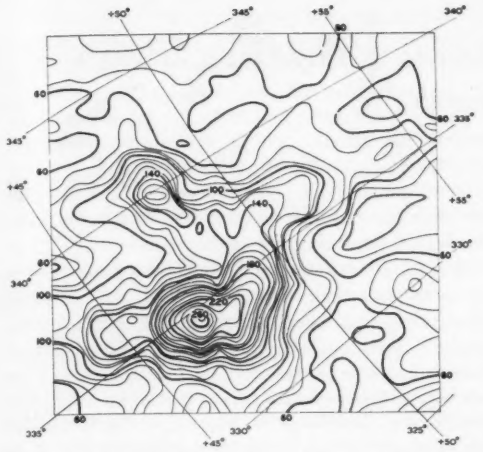


FIG. 7. Detail of large cloud in Area 1. The contours represent numbers of nebulae per square degree but are based on smoothed counts per quarter square degree.

When we examine the contour maps in figures 4 and 5, it is evident that on a smaller scale the density is not constant. The great clouds at $l = 334°$, $b = +47°$ (fig. 4), and at $l = 274°$, $b = +47°$ (fig. 5), are certainly real. The number of nebulae in each is of the order of a thousand. They apparently differ from ordinary clusters in that they are larger in over-all dimensions and contain several centers of condensation. Contour maps of these clouds were prepared on the basis of counts by quarter square degrees. These are shown in figures 7 and 8. The cloud in Area 1 (fig. 7) has two prominent centers of condensation while that in Area 2 (fig. 8) has three. Dr. Mayall has obtained spectra of two nebulae in the first cloud. The red shift amounts to about 11,000 km/sec corresponding to a distance of over sixty million light years. If we estimate the apparent diameter of the cloud to be 10 degrees, the linear diameter is more than ten million light years. This is probably larger than any known cluster of nebulae. The order of this diameter, together with the characteristic of multiple condensations, indicates that we may be dealing here with a different type of nebular aggregation than has previously been recognized.[5] A more detailed investigation of this cloud by Dr. Mayall and the senior author is now under way. No estimate of distance for the cloud in Area 2 is at present available, but it appears to be similar in all essential respects to that in Area 1.

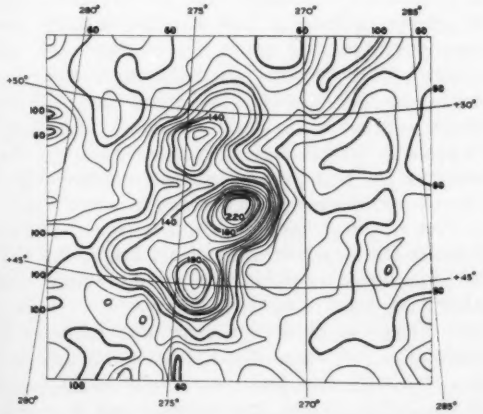


FIG. 8. Details of large cloud in Area 2. The contours represent numbers of nebulae per square degree but are based on smoothed counts per quarter square degree.

Figures 4 and 5 give evidence of other but less conspicuous aggregations of nebulae. It may be that the crowded contours running off the chart at $l = 347°$, $b = +58°$ (fig. 4) represent a portion of another large cloud. Several of the smaller condensations shown in figure 4 may represent physically associated aggregations of nebulae.

To summarize this discussion of nebular distribution, we may say that the evidence concerning inhomogeneities of very large scale is indecisive though tending to be negative. Smaller-scale groupings with diameters of a few million light years or less apparently are not uncommon. It is within the range of possibility that most of the nebulae may belong to such aggregations.

In the course of this work, Dr. Mayall counted the nebulae on several plates and made numerous fruitful suggestions which we gratefully acknowledge.

[5] A similar type of cloud may be represented by the aggregation of nebulae to which attention has been called by Tombaugh. Tombaugh, Clyde W., The great Perseus-Andromeda stratum of extragalactic nebulae and certain clusters of nebulae therein, as observed at the Lowell Observatory, *Publ. Astron. Soc. of the Pacific* **49**: 259–263, 1937.

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LOUIS B. WRIGHT

Director, Folger Shakespeare Library

(*Read April 22, 1949*)

OF ALL the qualities possessed by the settler in America, from Jamestown in 1607 to San Francisco in 1849, the most persistent was perhaps his optimism. If he did not believe that the wilderness or desert in which he found himself was precisely the best of all possible worlds at that particular moment, he tried to convince himself that his land would soon be an earthly paradise. If it is not literally true that every American was an optimist—for in all periods one encounters a few bitter souls, disillusioned and hopeless—nevertheless optimism characterized the period of American territorial expansion. Notwithstanding the evidence in the literature of disillusionment of our own generation, this cheerful quality remains a dominant trait of Americans as a whole. An analysis of optimism in its varying kinds and degrees during our past history would help explain many attitudes which have shown amazing persistence in American character.

The metaphysical and philosophical definition of optimism, as given by Webster, is the belief that since everything in nature is the work of God, all is ordered for the best. Under a secondary meaning, optimism is defined as a "disposition to take the most hopeful view." The latter definition characterizes the American frontiersman better than the first because the settler undoubtedly was skeptical about the goodness of the aborigines and other intractable evidences of God's handiwork. Nevertheless the frontiersman was not without belief in a benign Providence that would help him make a prosperous and pleasant home in the country of his choice however unfavorable conditions might appear at the moment. No hardships were so great that he could not overcome them with hard work and what he unsentimentally described as "good luck." His optimism was an article of faith, faith in himself, faith in his future, and faith in the environment which he was determined to mould to his liking. If the spot which he selected proved unfavorable, he moved to a better place. He rarely lost faith in an ultimate achievement of happiness and prosperity. Over the horizon lay the new Eden which he would sooner or later conquer as his own.

The first settlers of the seventeenth century brought with them certain concepts and attitudes which they had inherited from the English Renaissance. Many of the more influential emigrants retained a little of the spirit of Faustus or of Tamburlaine: the infinite was attainable; the New World was their oyster. For all of the hardships, bad management, and mishaps at Jamestown, Captain John Smith foresaw the beginning of an English empire in America. Even though death stalked the mosquito-ridden peninsula and the grim starvation of 1609 was only four years past, the Reverend Alexander Whitaker wrote from Jamestown in 1613 his *Good Newes From Virginia,* extolling the land as a paradise bountiful beyond belief. His description of the multitude of fish, many of which he had caught "with mine angle," was alone sufficient to send Izaak Walton's father's generation trooping to Virginia. His little book was at once an idyl and a shrewd piece of promotion literature. The Reverend Francis Higginson, writing of New England in 1630, was equally enthusiastic about the goodness of that country. The very air was balmy and salubrious so that he had "now cast away my Cap, and doe weare none at all in the day time." His *New-Englands Plantation* (1630) must have cheered many an emigrant who read its recommendation "for all cold complections to come to take Physicke in New-England: for a sup of New Englands Aire is better then a whole draft of old Englands Ale." No cynic arose to point out that the author died within the year from tuberculosis.

A strong sense of optimism was inherent in Puritanism. So powerful has been the influence upon American imaginations of the visual image of a grey-clad and somber Puritan, that it may be difficult to conceive of the Puritans as early American optimists, but their faith in the country and themselves was unshakeable. They knew that they were God's elect and that they were destined to inherit the earth, not only of New England, but

later of the Western Reserve and successive territories in the West. They came nearer than any other Americans to an acceptance of the metaphysical definition of optimism. If everything in the new land was not already ordered for the best, they assuredly were God's chosen instruments to make any required improvements. Their very hardships proved a source of cheering inspiration, if we may believe Edward Johnson's *Wonder-Working Providence of Sions Savior* (1628). Of a hurricane which might have caused less stout-hearted souls to quail, Johnson comments:

> But the Lord Christ intending to make his New England Souldiers the very wonder of this Age, brought them into greater straites, that this *Wonder working Providence* might the more appeare in their deliverance, for coming a shipboard, and hoiseing saile to accomplish their Voyage, in little time after they were tossed and sore beaten with a contrary winde, to the losse of the Ships upper worke, with which losse and great perill they were driven back againe, the Lord Christ intending to confirme their Faith. . . .

The faith and optimism which Puritanism engendered in the faithful lingered in their descendants long after the earlier religious fervor had weakened. New Englanders had from the beginning a sense of Manifest Destiny.

Their sense of destiny and their faith in their call to conquer the wilderness was equalled only by that of another great group of Calvinists, the Scotch-Irish, who, in the eighteenth century, succeeded the New England Puritans as the zealots of the frontier. With a cheerful disregard of danger and hardship, these people spearheaded the drive over the Alleghenies and the western slopes of that mountain area. They displayed an assurance, a faith, and a hope which sometimes annoyed less pugnacious and less destiny-conscious folk.

A part of the optimistic spirit in expanding America was of course neither a result of Renaissance or Puritan inheritances but was simply a manifestation of exuberant hope which the opportunities of a new country presented. As in the case of Milton's Adam and Eve—those first pioneers—"the world was all before them." A man with courage, and reasonable luck, could prosper and improve his lot.

Much of the writing which reflects the optimism of the American in his push westward can be described as promotional literature, but it cannot be dismissed as merely that. From colonial times onward, some of it achieved genuine importance as the sincere expression of devoted and competent writers. Much of it was vastly more influential upon American thinking than anyone suspects. And a little of it, as for example, writings on this theme by Emerson and Walt Whitman, became a part of classic American literature.

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Of all the publicists none was more shrewd, more convincing, and more sincere than William Penn. His writings about the opportunities in his colony deserve careful study, not only by advertising writers, but by everyone who would understand the psychology of seventeenth-century promoters and emigrants.

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Christians. Those Europeans already resident in Pennsylvania, principally of the Dutch and Swedish nations, were blessed with numerous healthy children, "some six, seven, and eight sons."

The clamor for news of this earthly paradise was so great that Penn brought out in 1685 *A Further Account of the Province of Pennsylvania and Its Improvements, for the Satisfaction of Those That Are Adventurers and Inclined to Be So.* Within a year this tract had two editions in English and one in Dutch. As in his earlier *Letter* Penn emphasized the opportunities awaiting every man to achieve freedom and prosperity in Pennsylvania. Like a modern real estate agent, he pointed out that the worst building lot in Philadelphia had increased within a few years four times in value, and some lots had increased forty times.

Penn's own pamphlets received confirmation from other writers who viewed the country and wrote with equal enthusiasm. The facts were that the Quaker proprietor had cannily understated rather than magnified the opportunities in the colony. Emigrants who landed there found that they could buy land at a reasonable rate and enjoy peace and freedom. To German emigrants from the Rhineland, victims of generations of devastating wars, Pennsylvania was indeed an earthly paradise. Englishmen who had suffered economic hardships and religious persecutions at home could hardly believe their good fortune. A little later the Scotch-Irish discovered that the port of Philadelphia was a gateway to an independence of which they had scarcely dreamed.

Human nature being what it is, the polyglot inhabitants of Penn's colony of course did not live together in complete bliss. We must remember that even in heaven, certain angels were sufficiently dissatisfied to revolt. But Pennsylvania spelled hope to most of its inhabitants. If they found that colony crowded, they moved on to neighboring territory, always with a faith in the future. Pennsylvania was a symbol of new hope and proof of the goodness of the New World. If some few were disgruntled, thousands of others received a fresh lease on life when they disembarked at Philadelphia. The literature which emanated from Pennsylvania in the seventeenth and eighteenth centuries testified to a solid foundation for the optimism of promoters and emigrants alike. Benjamin Franklin is the best known example in the eighteenth century of the success which Pennsylvania promised, but there were many others who proved equally well that Pennsylvania was promises realized. In such an atmosphere the pessimist could retain his gloom only by the exercise of the utmost perversity.

Although Pennsylvania offered the most dramatic—and the best publicized—opportunities, other colonies were not far behind in providing a hopeful milieu. Even where conditions were hard, the inhabitants always felt that over the border lay something better. The back country of North Carolina, if we can believe William Byrd, was largely peopled by hopeful, though sometimes dishonest, migrants from Virginia.

Many a colonial narrative asserted, and many a colonist believed, that the earthly paradise would be discovered somewhere in the domain marked out for British settlement. Edward Bland, an English trader resident in Virginia, published in 1651 *The Discovery of New Britain* extolling a country to the west of the settled portions of Virginia. Bland's preface to this work assured his readers that they would be furthering God's own designs by settling in the new country between the thirty-fifth and thirty-seventh parallels of north latitude. This region, he suggested, was particularly desirable, and he quoted in proof a statement from Sir Walter Raleigh's *Marrow of History* showing that God had placed Eden in this latitude. Without much doubt settlers beyond the Blue Ridge could expect a repetition of Eden. The search for Eden or its equivalent—and what is more important, the belief in the attainment of the earthly paradise—very early became a part of the American dream. When William Byrd described an inspection of some of his backwoods real estate he called his narrative *A Journey to the Land of Eden,* and when he supplied notes for a promotional tract to lure German Swiss to buy his frontier holdings, he called the pamphlet *Neue-Gefundenes Eden.* By Dickens' time, the talk about Eden had become more than a little trite and a subject for satire in *Martin Chuzzlewit.* Nevertheless, for a century before Dickens the word Eden had described what many Americans believed they would find in their push westward.

As Americans moved across the Alleghenies in the late eighteenth century and began their march across the Mississippi Valley in the early nineteenth century, they were buoyed by an invincible hope that no hardship could destroy. The muddy little settlements on each side of the Mississippi which depressed English observers and critics meant something more to the inhabitants. They saw in the shabby settlements of Lexington, Frankfort, or Cincinnati great cities which would one day rise in place of hovels. Many of them

also saw the repetition of older cultures which they sought to imitate or duplicate. One has only to read the newspapers, the pamphlets, and the books printed in these frontier towns to comprehend the earnestness of these inhabitants and the reality of their faith. The intellectual hopes of these frontier communities were illustrated in the schools and colleges which the settlers established, sometimes before they themselves had decent houses. Although the college may have been little more than a name, two or three teachers, and a makeshift building, it typified the cultural hopes of the community. Believing as they did in the future, seeing beyond the crudities of the present, these people were naturally hurt when critics like Mrs. Trollope and her successors ridiculed them —sometimes for the very earnestness of their aspirations.

The student who would understand American character would do well to read widely in the literature of the Middle Western frontier which Professor Ralph Rusk has so painstakingly listed for our guidance.[1] The student would also do well to watch for studies which Miss Ada Nisbet has underway of English travellers' criticism of America and a complementary study of American defense against foreign criticism which Professor Merle Curti is preparing.

The sense of destiny which made Americans believe that they had a God-given right to all the lands which they wanted to the westward resulted in that political philosophy known as Manifest Destiny. This philosophy came to fruition in the 1840's; but as Mr. Albert Weinberg has shown in his exhaustive study of the subject,[2] the seeds of the doctrine were sown much earlier. Indeed, Manifest Destiny had its beginning even earlier than Mr. Weinberg suggests. The essence of the idea is found in that faith and hope of the first colonists which we have already discussed. In 1621, the Reverend Lewis Hughes, an Anglican minister in Bermuda, published *A Plaine And True Relation Of The Goodnes Of God towards the Sommer Ilands* to show "the goodnes of God in reseruing and keeping these Ilands, euer since the beginning of the world, for the English Nation, and in not discouering them to any to inhabit but to the English." Although God had seen fit to allow Indians and Mexicans to live in parts of the Great West, many Americans were certain that the Almighty planned for white men from the United States to occupy and rule the whole land between the Atlantic and the Pacific.

Although many of the emigrants who prodded their weary oxen across the Great Plains did not make articulate their own belief in Manifest Destiny, few of them lacked a sense of superiority and divine approval. This faith upheld them in times of bitter discouragement and physical suffering. Scores of journals and diaries testify to the faith in the goodness of God and their own ultimate material success. Some of these journals are crude jottings; others achieve the dignity of prose poems.

Every American ought to read the journal of Sarah Royce, mother of Josiah Royce the philosopher. This journal, written in 1849 and published by the Yale Press in 1932 as *A Frontier Lady*, is typical of the sublime optimism which gave nineteenth-century Americans courage to attempt the dreadful Carson Sink, or some other forbidding desert, and to seek a new home in the land beyond. Something more than a greed for gold in California buoyed them up.

Mrs. Royce, who crossed the desert with her husband and a two-year-old daughter, never once complains of the hardships. Death was imminent more times than one, but she was sustained by a belief that Divine Providence intended for them to succeed and would interpose no hardship too great for them to endure. When they nearly perished from hunger and thirst, she writes: "I had known what it was to *believe* in God, and to pray that He would never leave us. Was it thus then, that when all other helpers failed, he came so near that I no longer simply *believed* in Him but *knew* His presence there, giving strength in whatever might come?" After superhuman effort, the Royce party got across the Carson Sink, and Mrs. Royce noted simply in her journal: "We had conquered the desert."

Exuberant optimism reached its climax with the American conquest of the Pacific Coast. Something inherent in California seems to produce exaggeration, whether in nature, man, or letters. From the beginning of the American occupation of California, that region excited imaginations. In a beneficent country of sun and flowers, Utopias would flourish and the wildest dreams of pioneers would come true.

God had taken particular pains to make life pleasant and easy in California, travellers declared. One reported that even the dew which settled on rose petals turned to sugar so that one gathered candy each morning like manna. So wholesome

[1] Rusk, Ralph L., *The literature of the middle western frontier*, 2 v., N. Y., Columbia Univ. Press, 1926.

[2] Weinberg, Albert, *Manifest destiny*, Baltimore, Johns Hopkins Press, 1935.

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Christians. Those Europeans already resident in Pennsylvania, principally of the Dutch and Swedish nations, were blessed with numerous healthy children, "some six, seven, and eight sons."

The clamor for news of this earthly paradise was so great that Penn brought out in 1685 *A Further Account of the Province of Pennsylvania and Its Improvements, for the Satisfaction of Those That Are Adventurers and Inclined to Be So.* Within a year this tract had two editions in English and one in Dutch. As in his earlier *Letter* Penn emphasized the opportunities awaiting every man to achieve freedom and prosperity in Pennsylvania. Like a modern real estate agent, he pointed out that the worst building lot in Philadelphia had increased within a few years four times in value, and some lots had increased forty times.

Penn's own pamphlets received confirmation from other writers who viewed the country and wrote with equal enthusiasm. The facts were that the Quaker proprietor had cannily understated rather than magnified the opportunities in the colony. Emigrants who landed there found that they could buy land at a reasonable rate and enjoy peace and freedom. To German emigrants from the Rhineland, victims of generations of devastating wars, Pennsylvania was indeed an earthly paradise. Englishmen who had suffered economic hardships and religious persecutions at home could hardly believe their good fortune. A little later the Scotch-Irish discovered that the port of Philadelphia was a gateway to an independence of which they had scarcely dreamed.

Human nature being what it is, the polyglot inhabitants of Penn's colony of course did not live together in complete bliss. We must remember that even in heaven, certain angels were sufficiently dissatisfied to revolt. But Pennsylvania spelled hope to most of its inhabitants. If they found that colony crowded, they moved on to neighboring territory, always with a faith in the future. Pennsylvania was a symbol of new hope and proof of the goodness of the New World. If some few were disgruntled, thousands of others received a fresh lease on life when they disembarked at Philadelphia. The literature which emanated from Pennsylvania in the seventeenth and eighteenth centuries testified to a solid foundation for the optimism of promoters and emigrants alike. Benjamin Franklin is the best known example in the eighteenth century of the success which Pennsylvania promised, but there were many others who proved equally well that Pennsylvania was promises realized. In such an atmosphere the pessimist could retain his gloom only by the exercise of the utmost perversity.

Although Pennsylvania offered the most dramatic—and the best publicized—opportunities, other colonies were not far behind in providing hopeful milieu. Even where conditions were hard, the inhabitants always felt that over the border lay something better. The back country of North Carolina, if we can believe William Byrd, was largely peopled by hopeful, though sometimes dishonest, migrants from Virginia.

Many a colonial narrative asserted, and many a colonist believed, that the earthly paradise would be discovered somewhere in the domain marked out for British settlement. Edward Bland, an English trader resident in Virginia, published in 1651 *The Discovery of New Britain* extolling a country to the west of the settled portions of Virginia. Bland's preface to this work assured his readers that they would be furthering God's own designs by settling in the new country between the thirty-fifth and thirty-seventh parallels of north latitude. This region, he suggested, was particularly desirable, and he quoted in proof a statement from Sir Walter Raleigh's *Marrow of History* showing that God had placed Eden in this latitude. Without much doubt settlers beyond the Blue Ridge could expect a repetition of Eden. The search for Eden or its equivalent—and what is more important, the belief in the attainment of the earthly paradise—very early became a part of the American dream. When William Byrd described an inspection of some of his backwoods real estate he called his narrative *A Journey to the Land of Eden*, and when he supplied notes for a promotional tract to lure German Swiss to buy his frontier holdings, he called the pamphlet *Neue-Gefundenes Eden*. By Dickens' time, the talk about Eden had become more than a little trite and a subject for satire in *Martin Chuzzlewit*. Nevertheless, for a century before Dickens the word Eden had described what many Americans believed they would find in their push westward.

As Americans moved across the Alleghenies in the late eighteenth century and began their march across the Mississippi Valley in the early nineteenth century, they were buoyed by an invincible hope that no hardship could destroy. The muddy little settlements on each side of the Mississippi which depressed English observers and critics meant something more to the inhabitants. They saw in the shabby settlements of Lexington, Frankfort, or Cincinnati great cities which would one day rise in place of hovels. Many of them

also saw the repetition of older cultures which they sought to imitate or duplicate. One has only to read the newspapers, the pamphlets, and the books printed in these frontier towns to comprehend the earnestness of these inhabitants and the reality of their faith. The intellectual hopes of these frontier communities were illustrated in the schools and colleges which the settlers established, sometimes before they themselves had decent houses. Although the college may have been little more than a name, two or three teachers, and a makeshift building, it typified the cultural hopes of the community. Believing as they did in the future, seeing beyond the crudities of the present, these people were naturally hurt when critics like Mrs. Trollope and her successors ridiculed them —sometimes for the very earnestness of their aspirations.

The student who would understand American character would do well to read widely in the literature of the Middle Western frontier which Professor Ralph Rusk has so painstakingly listed for our guidance.[1] The student would also do well to watch for studies which Miss Ada Nisbet has underway of English travellers' criticism of America and a complementary study of American defense against foreign criticism which Professor Merle Curti is preparing.

The sense of destiny which made Americans believe that they had a God-given right to all the lands which they wanted to the westward resulted in that political philosophy known as Manifest Destiny. This philosophy came to fruition in the 1840's; but as Mr. Albert Weinberg has shown in his exhaustive study of the subject,[2] the seeds of the doctrine were sown much earlier. Indeed, Manifest Destiny had its beginning even earlier than Mr. Weinberg suggests. The essence of the idea is found in that faith and hope of the first colonists which we have already discussed. In 1621, the Reverend Lewis Hughes, an Anglican minister in Bermuda, published *A Plaine And True Relation Of The Goodnes Of God towards the Sommer Ilands* to show "the goodnes of God in reseruing and keeping these Ilands, euer since the beginning of the world, for the English Nation, and in not discouering them to any to inhabit but to the English." Although God had seen fit to allow Indians and Mexicans to live in parts of the Great West, many Americans were certain that the Almighty planned for white men from the United States to occupy and rule the whole land between the Atlantic and the Pacific.

Although many of the emigrants who prodded their weary oxen across the Great Plains did not make articulate their own belief in Manifest Destiny, few of them lacked a sense of superiority and divine approval. This faith upheld them in times of bitter discouragement and physical suffering. Scores of journals and diaries testify to the faith in the goodness of God and their own ultimate material success. Some of these journals are crude jottings; others achieve the dignity of prose poems.

Every American ought to read the journal of Sarah Royce, mother of Josiah Royce the philosopher. This journal, written in 1849 and published by the Yale Press in 1932 as *A Frontier Lady*, is typical of the sublime optimism which gave nineteenth-century Americans courage to attempt the dreadful Carson Sink, or some other forbidding desert, and to seek a new home in the land beyond. Something more than a greed for gold in California buoyed them up.

Mrs. Royce, who crossed the desert with her husband and a two-year-old daughter, never once complains of the hardships. Death was imminent more times than one, but she was sustained by a belief that Divine Providence intended for them to succeed and would interpose no hardship too great for them to endure. When they nearly perished from hunger and thirst, she writes: "I had known what it was to *believe* in God, and to pray that He would never leave us. Was it thus then, that when all other helpers failed, he came so near that I no longer simply *believed* in Him but *knew* His presence there, giving strength in whatever might come?" After superhuman effort, the Royce party got across the Carson Sink, and Mrs. Royce noted simply in her journal: "We had conquered the desert."

Exuberant optimism reached its climax with the American conquest of the Pacific Coast. Something inherent in California seems to produce exaggeration, whether in nature, man, or letters. From the beginning of the American occupation of California, that region excited imaginations. In a beneficent country of sun and flowers, Utopias would flourish and the wildest dreams of pioneers would come true.

God had taken particular pains to make life pleasant and easy in California, travellers declared. One reported that even the dew which settled on rose petals turned to sugar so that one gathered candy each morning like manna. So wholesome

[1] Rusk, Ralph L., *The literature of the middle western frontier*, 2 v., N. Y., Columbia Univ. Press, 1926.

[2] Weinberg, Albert, *Manifest destiny*, Baltimore, Johns Hopkins Press, 1935.

was the country that when Californians heard of a case of chills and fever—brought in from Missouri—they travelled miles to observe the victim's shaking.[3]

The earthly paradise, which American adventurers had sought since John Smith's time, was about to be realized in California. Though gold had been the first stimulus to excitement over the Pacific slope, a deeper interest than that in gold focussed attention upon California from the mid-nineteenth century onward and finds reflection in much writing of the period. The march of Americans to the Pacific signified the rise of an American empire and the culmination of a cycle of history, as Professor Henry Nash Smith has pointed out so ably in his article on "Walt Whitman and Manifest Destiny." [4]

The hopes and aspirations inspired by westward expansion were almost mystical in fervor. Whitman achieved the highest literary reflection of this phase of Manifest Destiny, but he was only one of a host of writers who drank from the heady flask of current patriotism. Whitman himself drew some of his inspiration from contemporary journalists and from that most unpromising of sources, Congressional oratory.

If Emerson lacked the blatancy of Whitman, he was not far behind in the enthusiasm which the new West inspired in him. His gospel of self-reliance and optimism preached in countless lectures—many in the West—strengthened an opinion which westerners liked to believe.

The student of American optimism will find it difficult to separate the genuine from the synthetic product, particularly when he is dealing with the Far West, and most particularly when he contemplates California. California can manufacture optimism as easily as a bottler makes soda-pop. This synthetic optimism, like soda-pop, often effervesces and goes flat with distressing speed, but there is always enough of it to bubble and spew with an extraordinary illusion of genuineness. The railroads, the real estate interests, chambers of commerce, and innumerable other organizations, civic and commercial, unite to convince the rest of America that California will provide the answer to their most extravagant prayers. Numerous Valhallas, of which the best known is Forest Lawn, promise that even Death is somehow different if it can be achieved in California. No relative need sorrow if the last rites of his loved one can be performed to the accompaniment of soft music, the song of canaries, and the flight of doves.

Though this pleasurable contemplation of Death —with its incidental inspiration of literature, not all of which is satirical—is a recent development, the propaganda of general optimism is as old as the American occupation of California. From Gold Rush days onward, a deliberate effort was made to "sell" California to the world by promising everything. A book by Professor Glenn S. Dumke, entitled *The Boom of the Eighties in Southern California* (San Marino, 1944), describes the lengths to which special interests like the railroads went in advertising the wonderland. Professional writers and historians became the willing instruments of propaganda. For example, Charles Nordhoff published a widely read and influential book called *California: for Health, Pleasure, and Profit* (1873), which in its time was credited with luring more people to California than any other single promotional work.[5] Nordhoff was in the pay of the railroads.

California was no frontier region, Nordhoff insisted. Indeed, the Far West outshone the dull and uncomfortable East, and for danger, difficulty, and discomfort New York was in reality the frontier which one might dread.

Even most of the satirists only half-mocked the New Eden. Charles Dudley Warner's article in the *Atlantic* called "Golden Hesperides," for instance, poked a little fun at the region but the total effect was that of favorable propaganda. This article gave currency to a favorite tale of the fertility of California soil, where melon vines grew fast and dragged the fruits along the ground at such a rate that "if you want to pick a melon in this country, you have to get on horseback."[6]

One can easily understand the reasons why California proved a favorable soil for a wide variety of Utopian projects, religious, political, economic, and a few so vaguely idealistic as to defy classification. No other region to my knowledge has ever seen the establishment of so many and so varied Utopias as California. One modified Marxist colony, the Kaweah group, is described in an illuminating article by Robert Hine, "A California Utopia: 1885–1890." [7] These colonists settled in Tulare County, in the neighborhood of

[3] Cleland, Robert G., *From wilderness to empire*, 178, N. Y., Knopf, 1944.

[4] *Huntington Library Quart.* **10**: 373–390, 1947.

[5] Dumke, Glenn S., *The boom of the eighties in southern California*, 30, San Marino, Calif., Huntington Lib., 1944.

[6] Quoted by Dumke, *ibid.*, 31.

[7] *Huntington Library Quart.* **11**: 387–405, 1948.

Sequoia Park, and pleased themselves by naming the biggest tree in the Giant Forest "Karl Marx Tree"—a tree which seems destined to have a name deplored by many Americans, for it is now called "General Sherman." A forthcoming book by Mrs. Marion Tinling of the Huntington Library will describe the Icarians, another interesting group of Utopians.

The belief in California as Utopia continues unabated, as newspapers, magazine writers, books, and returned G. I.'s who trained there, spread the gospel that all of one's dreams will come true under California's sun. A few sophisticates like Evelyn Waugh in *The Loved One* may satirize the grossest of Southern California's inanities, but few except other sophisticates ever read the satires. Southern California today demonstrates the ultimate in American optimism, the consummate faith in the attainment of the Good Life, whatever one's definition of that vague dream may be.

Our consistent belief in our own country as the New Found Eden, our infinite faith in our own ability to make the country into an earthly paradise, idyllic Utopia, or simply the safest, most comfortable, and most prosperous country on earth, have given amazing tenacity and strength to American determination to accomplish these very ends. We have believed in the country and in ourselves, and we have gone a long way toward realizing our dreams.

But optimism carries with it hazards as Voltaire long ago pointed out in the character of Dr. Pangloss. Too often Americans have approached a Panglossian philosophy. Californians and Texans especially should be required to read *Candide* with their morning prayers. Good as are those commonwealths, they are not yet the best of all possible worlds, and the complacence of citizens who so regard them constitutes a genuine handicap to intellectual, spiritual, and social progress.

Our incurable optimism has had unfortunate results in international politics. It has been fundamentally responsible for much of our isolationism. The student of this attitude in American foreign relations will do well to contemplate the Pollyanna quality in American thinking. We are big, strong, and safe. We have never lost a war. One American can whip ten "foreigners." A million men will spring to arms over night if we are attacked. These are heresies of hope which have led us into incredible errors.

So convinced are many Americans of the perfection of the particular forms of government and society evolved in the United States that they have a naive assurance that the world's difficulties could be solved by encouraging the export and the immediate acceptance of American social and political ideals by people as different as the Italians, the Chinese, the Japanese, and the islanders of the South Pacific.

Historically, optimism has supplied a cheerful and comfortable set of attitudes for Americans. Optimism has dominated American thinking as we have expanded into a great nation. The historian might perform a useful service by examining the validity of our traditional attitudes in the light of the constrictions which now apply to Americans as well as to the rest of the modern world.

ON THE ELECTRICAL POTENTIALS OF AN EARTHWORM, EISENIA FOETIDA, CONSIDERED IN RELATION TO GROWTH[1]

GAIRDNER B. MOMENT

Professor of Biology, Goucher College, Baltimore, Md.

INTRODUCTION

PREVIOUS work has established the fact that in the earthworm, *Eisenia foetida*, the number of segments regenerated posteriorly is numerically limited. No matter what the level, from the 30th to the 80th segment, at which the posterior part of the worm is amputated, the regeneration of new segments stops when the large old segments plus the small new ones summate to approximately 90 (Moment, 1946).

This limitation proved to be independent of the length of the worm in millimeters or of the original number of segments (a group of worms originally possessing more than 90 segments will regenerate, on the average, the same number of segments as a group selected for having fewer than 90, provided only that all are cut at the same level). These facts led to the conclusion that

> whatever may be the force or influence that finally stops the formation of new segments out at the end of the worm, it must fulfill certain conditions. It must be independent of the size of the units producing it (since 50 large old segments plus 40 small new ones have the same result as 80 large old ones and 10 small new), but closely dependent on the number of those units in linear series. Its action must attain its full force at the very end of the series. The well known force that fulfills all these requirements is the electromotive force of a series of galvanic cells [Moment, *ibid.*].

It has long been known that an applied e.m.f. will inhibit growth (Lund, 1921; Barth, 1934*a*). Therefore it seemed reasonable to propose (Moment, *ibid.*) that "Proliferative growth in length takes place by the addition in series of new anatomical units until, by addition, a critical inhibitory voltage is built up at the growing end which prevents further cell division."

If the proposed theory is correct, large and small worms with similar numbers of segments must have similar voltages. If they do not, the theory is untenable. If they do, the theory is shown to be possible but is not thereby proven. This survey of the electrical characteristics of *Eisenia foetida*, an earthworm much used in regeneration studies, was made (1) to determine whether or not the theory was a possible one, and (2) to establish norms for use in any future experimental work.

PREVIOUS WORK

Previous work on the electrical polarity and voltage of earthworms is meager. The earliest study is that of Morgan and Dimon (1904). They reported that freshly cut ends were usually negative, that when two ends were cut simultaneously, the anterior end was usually positive when the piece was short, but negative when the piece was long, and that a regenerating tip was usually positive to old tissue. They drew no conclusions from these diverse facts except to say that they "need further investigation."

Two decades later Hyman and Bellamy (1922) in the course of work chiefly on coelenterates reported that Bellamy had "verified" the results of Morgan and Dimon, on an earthworm but give no details. For polychaetes they reported on a basis of 23 individuals that "in *Nereis*, after being kept in the laboratory, anterior and posterior ends are electropositive to the middle."

The most extensive previous study of electric potentials, and, in fact, the only other in addition to those just mentioned, is that of Watanabe (1928). His methods were similar to those used in the present work except that he employed $Zn\text{-}ZnSO_4$ electrodes, an older type of potentiometer, and no anaesthetic. He reported not only the electrical polarity but also the actual voltages recorded between anterior and posterior ends as well as two intermediate points on 100 adults of the Oriental earthworm *Perichaeta communissima*. Of 50 individuals in which the voltage was determined with the worm right side up, the tail end was positive to the head in 48 cases, and

[1] A grant from the Penrose Fund of the American Philosophical Society, which supplied the electrical equipment used, is gratefully acknowledged.

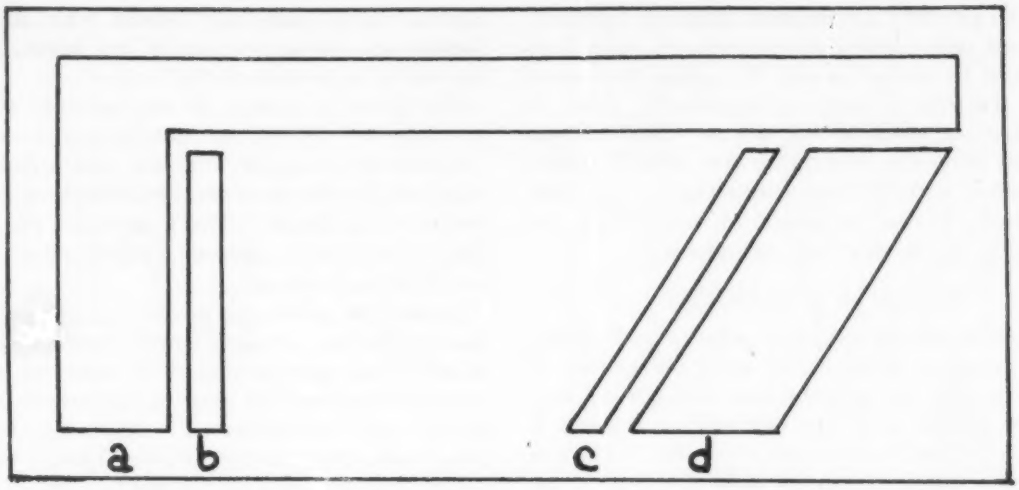

FIG. 1. Diagram of paraffin block with four troughs of saline as used in initial measurements.

the mean voltage, calculated from his data, was 12.5 millivolts with a maximum of 28 and a minimum of − 11.9 MV. The values for the 50 individuals measured ventral side up were similar.

MATERIALS AND METHODS

The common brandling or manure pile earthworm, *Eisenia foetida*, used in earlier experiments, was employed throughout. To reduce variability, all methods of handling were standardized. The worms were lifted with metal forceps gently slipped under them near the anterior end. Attempts to use cold to quiet the worms proved unsuccessful because they continued to move at less than 1 degree Centigrade. Consequently, 0.2 per cent chloretone was employed as an anesthetic as used by Burr and Hovland (1937) in their work on bioelectric potentials in the salamander larva. Adults were removed from the chloretone as soon as they became quiescent but never in less than 3 or more than 4 minutes after immersion, and placed on a piece of paper towel for about 20 seconds. They were then placed on a second piece of paper towel which was put on a glass plate and pushed under the electrodes.

The electrodes were Ag-$AgCl_2$ spirally wound wires prepared according to Burr (1943). Each electrode was placed in an ordinary medicine dropper pipette filled with physiological saline and plugged with a camel's hair brush trimmed to a fine point. The electrode pipettes were mounted in adjustable holders made from the coarse adjustments of obsolete microscopes. These electrodes usually had a potential difference of 2 to 3 millivolts which tended to fluctuate slowly so that the difference between electrodes was determined before and after each measurement. A reversing switch in the leads to the electrodes made it possible to reverse the connections to the worm without moving the animal itself.

By the tail being electropositive to the head, it is meant that when one electrode brush is on the head and one on the tail, the one on the tail must be opposed to the plus terminal of the potentiometer in order to balance the potentiometer voltage and give a null reading in the galvanometer. The potentiometer was a Leeds and Northrup type K2 and the galvanometer a type HS. The two together provided a theoretical sensitivity of 0.5 microvolt. In actual practice this set-up discriminated with ease down to plus or minus 5 microvolts, a much greater sensitivity than needed in material varying in the millivolt range.

The voltage was read for each worm as soon as the electrode brushes were applied and then at intervals up to four minutes after the worm had been removed from the chloretone. Very soon thereafter the worms began to dry up and become active. During the four minutes the voltage usually but not always increased. The maximum voltages observed were always recorded. Recording maximum voltages was the procedure that gave significant results in the study by Coates and Cox (1945) of the correlation between increase in voltage with increase in number of electroplaxes in the growing electric eel.

In the early measurements the worms were spread out on a paraffin block, similar to that used

by Barth (1934*b*) for measurements on hydroids, with their ends dipping into troughs of physiological saline (*a* and *d* in fig. 1). Two additional empty troughs (*b* and *c*) supposedly aided in insulating the ends of the worm. The voltages recorded from the worms on the paraffin blocks were more variable and considerably less than those from worms on paper. Consequently the paraffin block method was abandoned.

POLARITY AND INJURY

To determine the electrical polarity of this species of worm, a series of 50 adult individuals of *Eisenia foetida* was anesthetized in the usual way and then placed under the electrodes. Because it was thought that the end near which the worm was lifted, however gently, might have an effect on the electrical polarity, alternate worms were lifted by opposite ends. The first 25 worms were extended on a slab of paraffin with each end dipping in a small depression filled with physiological saline. The second 25 were simply extended on a paraffin slab. The electrical polarity was then determined by the method already described. After this initial determination, each worm was picked up by the previously untouched end, replaced on the paraffin facing in the opposite direction and the polarity again determined.

In 48 of the 50 cases the tail was electropositive to the head in both readings. In only one case was it negative in both readings, and then but slightly. In one case it was positive in the first but negative in the second reading. In fact it was found that in all but very rare cases such as the two above, these worms can be repeatedly picked up, handled, and replaced without upsetting their electropolarity, i.e., tail end positive to head end.

Measurements made in the course of subsequent work on over 300 individuals gave 286 in which the tail was positive and only 14 in which it was negative to the head end. These exceptional individuals were found almost entirely among large adults and never among newly hatched or regenerating worms. In some cases the difference in voltage was very slight so that although the tail was negative to the head, it was not much so. In all the 6 cases in which exceptional worms were put aside and redetermined on a subsequent day, the tail was positive, indicating that the originally recorded polarity had been due to some transitory condition.

The electrical polarity was also determined for 10 individuals of the common *Lumbricus terrestris*, and for 8 *Octolasium lacteum*, the common grey earthworm of woodland humus. In all of the former and in all but one of the latter, the tail proved to be positive to the head.

Partly as a control on the possible effect of handling and injury, and partly to find out what the polarity would be after the kind of cut from which regeneration occurs, two additional series of worms were tested. There were 10 good sized (*ca.* 75 mm) and vigorous individuals of *Eisenia foetida* in each series.

In the first series the polarity and the potential was determined for each worm, then a quarter of an inch of the anterior end of the worm cut off with a sharp scalpel and the potential difference between the two ends redetermined. In every case both before and after cutting off the "head," the head region was negative to the positive tail. But in every case it had become more negative than before, as was to be expected since injured surfaces are well known to be negative to uninjured. Consequently there was an increase in e.m.f. between the two ends of the worm, shown as a rise in voltage averaging slightly over 3 MV (see table 1).

TABLE 1

CHANGE IN VOLTAGE BETWEEN ANTERIOR AND POSTERIOR ENDS AFTER CUTTING OFF A QUARTER INCH FROM HEAD OR TAIL

		Rise in voltage	Fall in voltage
Head cut	Maximum	13.04 MV	
	Minimum	.15 MV	
	Mean	3.02 MV	
Tail cut	Maximum		6.14 MV
	Minimum		.11 MV
	Mean		2.80 MV

In the corresponding series of worms in which a quarter of an inch was cut off from the posterior end of the worm, and the polarity and voltage redetermined, similar results were obtained. Both before and after the cut, the tail end was positive to the head. However the tail was less positive than before. The average decline in voltage was 2.8 MV (see table 1).

VOLTAGE OF INTACT ADULTS

To explore the actual potential difference between the two ends of normal worms, 5 groups of 10 adults each were used. The means of the 5 groups varied considerably and the values for the individuals even more so. The 5 group means, each based on 10 animals, were 15.60, 12.46, 17.90,

10.43, and 11.98 MV. The grand mean for all 50 worms was 13.67 MV with the tail end again positive to the head. This value is close to the 12.5 MV obtained by Watanabe (1928) from 50 Japanese earthworms.

The variability, which Watanabe also found, seemed to warrant further investigation. For this purpose 20 good sized adults without clitellums were selected. Their mean length was 71.4 mm., with a maximum of 90 and a minimum of 45 mm. Their mean number of segments was 94.9, with a maximum of 106 and a minimum of 80. Each worm was isolated in a separate glass finger bowl of earth and kept at a room temperature near 23 degrees Centigrade. The voltage of each worm was determined on ten different days over a two month period.

The mean value for these 200 determinations was 15.1 MV with a standard deviation of 9.5 and a standard error of the mean of 0.68. In other words, there are 95 chances in 100 that in any subsequent similar set of readings the mean would lie within plus or minus 1.36 MV of 15.1.

TABLE 2

THE MEAN VOLTAGES FOR EACH OF 20 WORMS

Individual worms	Mean voltage (millivolts)	Individual worms	Mean voltage (millivolts)
1	18.52	11	21.73
2	11.58	12	11.64
3	24.83	13	15.73
4	11.34	14	14.28
5	9.57	15	21.17
6	21.88	16	12.26
7	8.15	17	15.38
8	11.76	18	9.47
9	17.9	19	14.95
10	20.74	20	9.43

The mean values for each of the 20 worms are shown in table 2. This seemed a more manageable way of presenting the data than giving every individual reading as was done by Watanabe for earthworms and by Barth (1934) for coelenterates. It will be evident from inspection that there is a great deal of variability extending from worm number 3 with a mean voltage of 24.83 MV to worm number 7 with a voltage of only 8.15 MV. Table 3 groups the worms according to the number of times each was above or below the grand mean. It will be seen that despite the fact that the means of so many individual worms were considerably above or considerably below the grand mean (table 2), nevertheless no worm was always above

TABLE 3

DISTRIBUTION OF VARIATION IN VOLTAGE AMONG 20 NORMAL ADULTS

Number of worms in each class	Number of times Below mean	Above mean
0	10	0
0	9	1
2	8	2
4	7	3
4	6	4
2	5	5
2	4	6
3	3	7
3	2	8
0	1	9
0	0	10

or always below the grand mean. In fact no worm was above 9 times and below only once or vice versa.

VOLTAGE OF NEWLY HATCHED WORMS

Some years ago Sun and Pratt (1931) showed for the first time that earthworms do not grow by the addition of new segments but emerge from the egg cocoons possessing, on the average, the full adult number. From this fact it should follow, if the theory under test in this paper is correct, that despite their diminutive size worms on emergence from the cocoon should have fully as great a potential difference between anterior and posterior ends as do adults.

Consequently the voltage of over 40 newly hatched worms was determined by a method like that used for the adults except that these very small worms were allowed to remain in the chloretone only one minute instead of three or four as with the adults. One minute was necessary to immobilize them but three minutes was obviously injurious. Furthermore, on these little worms a binocular dissecting microscope was used to make certain which end was anterior and which posterior. With a microscope it was also easy to double check on the species because the characteristic transverse dark brown bands are already visible, especially on the anterior segments of the wormlings.

The worms were allowed to emerge from their egg cocoons in a Petri dish with moist filter paper. None was more than a week old, counting from the time of emergence, and many only 24 hours old when the voltage was measured. It is not likely that the actual time of emergence has much significance because the lengths of the worms in millimeters vary greatly at that time, from a

maximum of 35 mm to a minimum of 8 mm in the groups studied, though the mean in each of the four groups of ten varied only from 24.27 to 19.50 mm. Some of this variation may be due to the amount of foodstuffs in different cocoons. In any case the cocoons differ greatly in size and also in the number of wormlings they contain which varies at least from one to four. The diameter of all the newly emerged worms, however, was very small so that they all appeared thread-like in comparison with adults.

TABLE 4

FOUR GROUPS OF TEN NEWLY HATCHED WORMS

	Segments	Lengths	Voltage
Maximum	108	32 mm	32.45 MV
Minimum	101	19	1.25
Range	7	13	19.95
Mean	104.2	24 mm	24.20 MV
Maximum	100	30 mm	35.75 MV
Minimum	86	16	−.66
Range	14	14	35.41
Mean	94.36	24.27 mm	19.51 MV
Maximum	110	35 mm	29.50 MV
Minimum	78	8	4.00
Range	32	27	25.50
Mean	94.59	19.50 mm	16.27 MV
Maximum		35 mm	39.05 MV
Minimum		12	9.35
Range		23	29.97
Mean		22.1 mm	19.70 MV
Grand Means	97.72	22.47 mm	19.92 MV

Table 4 shows the means, the maxima, the minima, and the range for segment number, for length in millimeters, and for voltages in four groups of ten wormlings each, together with the grand means.

It will be seen at once, that the results of Sun and Pratt are fully confirmed. These newly hatched worms possessed, on the average 97.72 segments as compared with 94.36 for a population of 811 taken from the wild (Moment, 1946).

The mean length, measured under chloretone, was 22.47 mm, about one-third the length of adults under similar circumstances. The diameter was so small that the accuracy of measurement was doubtful but most of the worms were just about 1 mm wide at the widest and many of them were less.

The electrical polarity was the same as in adults in every case, i.e., the posterior end electropositive to the head.

The voltages recorded seemed surprisingly high. The mean value for all 40 worms was 19.92 MV which is well above that of the adult which averages about 15 MV. Even the group with the lowest mean value averaged 16.27 MV. Whatever the reason, there seemed no doubt about the fact of these relatively high values. The kick that these little worms gave to the galvanometer was amazing to see. Since it was realized at the time of measuring that the value in the first group was high as compared with the adults, the second group was measured after double checking on the accuracy of the apparatus. When this again gave the same results, the third and fourth groups were measured at intervals of several weeks but they each also gave high mean voltages.

DISCUSSION

The critical inhibitory voltage theory of growth limitation that initiated the present survey would appear to require, if it is correct, that earthworms show a constant electric polarity. It would also require that the actual voltage depend on the number of segments rather than the lengths of the worms.

The electric polarity is easy to determine by the method used and is not upset by gentle handling of the worms. The little group of aberrant individuals (in the later determinations about two in 100) in which the polarity is reversed present a puzzle. The only explanation at hand is the effect of some unnoticed injury. This explanation is supported by the fact that in every case where a worm with a reversed polarity was isolated and reexamined after a period of a day or so the polarity had returned to normal. Whatever had caused the reversal was some temporary condition.

The polarity of the three kinds of North American worms tested was the same as that found by Watanabe (1928) for the Oriental earthworm, *Perichaeta communissima*. Furthermore, it has been found by Burr and Lane (1936) that the growing tail ends of salamander and mouse embryos are positive to the head. Similarly Lund (1947) has found the growing tips of onion roots and fir trees to be electro-positive to the rest of the organism.

The voltages registered by the intact adults, a mean of 13.67 MV in one group and 15.1 MV in a second, making a total of 250 determinations, is

not very different from the 12.5 MV obtained by Watanabe in the only comparable study.

The variability is undoubtedly high and presents an unanswered question. To point out that a comparable degree of variation has been reported by all other investigators in this general field is true but not very enlightening. Barth (1934*b*), Burr and Lane (1936), Lund (1947), and Osterhout (1934), all have reported it.

In *Eisenia foetida* the number of segments varies considerably within an unselected group. In a population of 811 with a mean segment number of 94.36, the standard deviation was 7.47 segments. However, this variation cannot be the principal explanation for the variation in voltage because the latter is found within one and the same worm in the course of a series of measurements.

There was only one other unexplained type of variation observed in these animals and there is a bare possibility that there is some obscure connection between it and variation in voltage. In any case, some worms react much more violently by squirming and writhing when placed in chloretone than do others even though all are the same in size and general condition and have been carefully handled in exactly the same manner.

The voltage recorded for the very small worms right after emerging from their egg cocoons was not only fully comparable to that of full grown adults but even greater. Several possibilities present themselves. Each voltage producing unit in the wormlings, whether those units be the segments or the cells, may produce a slightly greater voltage than is the case with adults. Or it might be that the thinner more permeable skin or the very small diameter combined with the same length of segments was responsible. There certainly can be no doubt about the vigor of the kick these little worms gave to the galvanometer.

The fact that the electric polarity of wormlings and adults is constant and the same in both supports the theory that growth in length is limited by the attainment of a critical inhibitory voltage. The theory finds further support in the unquestionable fact that when the young worms hatch they possess not only their full adult number of segments but, small as they are, a voltage even greater than that of the average adult.

SUMMARY AND CONCLUSIONS

1. The electric polarity of over 160 individuals of the common manure pile earthworm or brandling, *Eisenia foetida*, was determined. Except for a very few aberrant individuals, the posterior end was electropositive to the anterior.

2. The same electric polarity was found in *Lumbricus terrestris* and *Octolasium lacteum*.

3. Neither gentle handling nor amputating either the anterior or posteriormost quarter inch of a worm upset this polarity.

4. The potential difference between the two ends in a group of 50 intact adult worms averaged 13.67 MV. In a subsequent group of 20 aclitellate intact adults with an average length of 71.4 mm and 94.9 segments, the grand mean of 200 determinations was 15.1 MV. These means compare with 12.5 MV obtained by Watanabe on a group of 50 Oriental earthworms.

5. The variability was of the same order of magnitude as that reported by Watanabe, Lund, Barth, and others for bio-electric potentials. However the standard error of the mean for the 200 readings on 20 worms indicated that there were over 95 chances in 100 that the mean of any subsequent similar set of determinations would fall within plus or minus 1.36 MV of the mean, 15.1 MV.

6. The mean number of segments in 40 newly hatched wormlings was 97.7, thus confirming the finding of Sun and Pratt that earthworms do not grow by the addition of new segments but emerge from their egg cocoons with the full adult complement.

7. The mean voltage of the 40 newly hatched wormlings was 19.9 MV.

8. The constancy of electrical polarity and the similarity of voltage between adults and wormlings with similar numbers of segments, support the critical inhibitory voltage theory of growth limitation.

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POSTGLACIAL MARINE SUBMERGENCE OF ARCTIC NORTH AMERICA WITH SPECIAL REFERENCE TO THE MACKENZIE DELTA

HORACE G. RICHARDS

Associate Curator of Geology and Paleontology, Academy of Natural Sciences of Philadelphia

INTRODUCTION AND ACKNOWLEDGMENTS

WHILE considerable work has been done on the Pleistocene deposits of the Eastern Arctic, very few studies have been made on those of the Western Arctic, and practically none on those in the vicinity of the Mackenzie Delta.

During the summer of 1948, I had the opportunity to make a brief reconnaissance trip to the delta of the Mackenzie River, for the purpose of obtaining information on the Pleistocene history of the region. This field work was aided by a grant from the Johnson Fund of the American Philosophical Society, and it is a great pleasure to acknowledge my thanks to the Society for making this work possible. I am also indebted to the Geological Survey of Canada for lending me specimens collected by J. J. O'Neill on the Canadian Arctic Expedition of 1913–1918.

We flew north from Edmonton, Alberta, to Aklavik, a small settlement in the Delta of the Mackenzie River, about seventy-five miles inland from the Arctic Ocean. From here we travelled in various directions throughout the delta region. For our longer trips, such as to Kittygazuit and Herschel Island, we chartered a Piper Cub Cruiser plane, while shorter trips were made by schooner and canoe to Black Mountain and nearby points in the Delta.

To give background to the discussion on the Pleistocene features of the Mackenzie Delta region, we propose to give a brief resumé of the late Pleistocene (post Wisconsin) history of the northeast and Arctic coast of North America, with special reference to the marine elements such as emerged strand lines and fossil deposits.

ATLANTIC COAST OF NORTH AMERICA

Recent work on the Pleistocene has demonstrated two theories or hypotheses that are of considerable importance in any discussion of the geological history of that epoch. One of these, sometimes called the "glacial control hypothesis," points out that since the water which formed the land ice must ultimately have come from the sea (because of the breaking of the normal cycle of flow-off and evaporation), sea level must have been lowered during the glacial stages. It is generally believed that at the climax of the last (Wisconsin) glacial stage, enough water was removed from the sea to lower sea level between 240 and 300 feet.

The second hypothesis concerns the effect of the weight of the Wisconsin ice sheets on the land. It is generally believed that the glaciated portions of North America, as well as other parts of the northern hemisphere, were considerably depressed by the weight of the ice sheets, generally more to the north where the weight of the ice was greater. As the glaciers began to melt, water was poured back into the sea, causing sea level to rise and flood the land which had been pushed down by the ice. Gradually the land began to recover from the load of the ice, and slowly rose.[1] Traces of the presence of this postglacial sea which occupied the depressed land before the land completely recovered from the ice load can be seen in the form of raised beaches or strand lines, frequently containing the fossil remains of shells and other marine animals. The total amount of crustal uplift is equivalent to the highest elevation at which marine features can be found plus an amount equal to the rise of sea level since the marine limit was recorded. The marine limit indicates the excess of the rise of the land over that of the sea since the retreat of the ice sheets (Antevs, 1939: 709). There is no evidence that the land south of the terminal moraine was very much depressed, if at all, by the weight of the Wisconsin ice.

The marine limit varies from 0 feet, twenty miles south of Boston, to 50 feet in Massachusetts north of Boston, 200–300 feet in Maine, and 100 feet in Labrador. The marine limit is fairly well marked along the Atlantic Coast by the presence of fossil shells, notably *Saxicava arctica*, *Leda gla-*

[1] Sherman Moore (1948) on the basis of accurate records of crustal movement in the Great Lakes region, believes that there is "no connection between the present movement and isostatic recovery from the weight of the ice." This argument has been refuted by Hobbs (1949).

MAP 1. Sketch map of Mackenzie Delta showing route of 1948 Expedition.

cialis, Macoma calcarea, M. balthica, and *Mytilus edulis.*

GULF OF ST. LAWRENCE

The late glacial sea extended up the St. Lawrence Gulf and River to Montreal with arms of sea water extending north along the Ottawa River to Ottawa, east to the vicinity of the Thousand Islands and south into Lake Champlain. This region must have been rather heavily depressed by the ice for the marine limit occurs at elevation 300 feet at Rivière du Loup, 500 feet at Montreal, 570 feet at Venosta, Ontario, and 690 feet near Ottawa. (Certain shoreline features not confirmed by fossils suggest an even higher marine limit of 717 feet near Montreal.) The fauna from Rivière du Loup and Montreal indicates fairly deep and saline waters, while those of Lake Champlain and the Ottawa River indicate shallow, brackish conditions. A transition between brackish and freshwater conditions is noted near Prescott, Ontario.

HUDSON AND JAMES BAYS

The thickness of the ice must have been fairly great in the Hudson Bay area, as demonstrated by the magnitude of the depression and subsequent rise of the land along the margins of that bay. Marine shells indicating a shoreline 250 feet above sea level have been found at Coral Rapids, Ontario, fifty miles south of James Bay, and fossil shells found on the beach at Charlton Island, seventy miles north of Moose Factory, indicate deeper and more saline water than that of James Bay today (Richards, 1936).

La Roque (1949) has recently suggested that there was a postglacial connection between James Bay and the Gulf of St. Lawrence, possibly by way of Lake St. John.

Farther north, a marine shoreline 450 feet above sea level has been found fifty miles inland from Churchill, Manitoba, while marine Pleistocene shells have been found at numerous places along both the east and west shore of Hudson Bay (Nichols, 1936; Richards, 1941). An arm of the sea extended up Chesterfield Inlet to Baker Lake, as indicated by marine mollusks found 100 feet above the level of that lake; similar arms of the sea probably extended up other inlets along the west coast of Hudson Bay, and it is quite probable that some of these extended northwestward to connect with the Arctic Ocean. Extensive submergence apparently occurred along Hudson Strait, for a marine limit up to 898 feet has been observed in this region (Nichols, 1936, 1936*a*, and personal communication).

ARCTIC ARCHIPELAGO

Apparently most or all of the islands of the Arctic Archipelago north of the Canadian mainland were glaciated at some time during the Pleistocene (Antevs, 1929: 652), while existing glaciers occur on some of the more northerly islands such as Prince Patrick and East Sverdrup islands. The submergence of portions of these islands by the postglacial sea is clearly indicated by the presence of raised strand lines, frequently with fossils, at varying elevations above the sea. Considerable information on the extent of the marine submergence in the Arctic is given in a recent work by Washburn (1947).

The highest recognizable shoreline feature on Victoria Island was found at 510 feet, although shell fragments were found up to 625 feet (near Cambridge Bay), while shells were found on Banks Island up to 500 feet elevation. The map accompanying Washburn's report summarizes the marine features (strandlines and fossil deposits) of the region. A few of these records based on earlier accounts indicate fossils up to "nearly 1,000 feet" on Cornwallis and Devon Islands (Sutherland, 1853: 300) and up to 2,000 feet above sea level on northern Ellsemere Island (Grinnell Land) (Greeley, 1888: **2**: 57). Some of the elevations may be open to question, but it is apparent that there was widespread submergence and later emergence in the Arctic Archipelago.

ARCTIC MAINLAND EAST OF MACKENZIE DELTA

Marine features are present on Kent Peninsula up to elevation 600 feet, along Coronation Gulf up to 500 feet and on Darnley Bay up to 600

feet (O'Neill, 1924; Washburn, 1947), indicating a considerable marine submergence in the region.

THE MACKENZIE DELTA

The only previously reported marine Pleistocene fossils from the region of the Mackenzie Delta are those collected by the Canadian Arctic Expedition 1913–1918 on Herschel Island and at Kay Point and listed by Dall (in O'Neill, 1924). Conditions along the Arctic coast from the Mackenzie Delta westward to Bering Sea differ in several important respects from those in the Eastern Arctic, and consequently the Pleistocene history of this region deserves special consideration.

MOVEMENT OF LAND INDEPENDENT OF THAT CAUSED BY WEIGHT OF ICE

Conspicuous earth movements are known to have taken place along the Pacific coast of North America during Pleistocene time continuing into the Recent. While there is no evidence that these diastrophic movements occurred in the region of Bering Strait and eastward along the Arctic Coast of Alaska, such movement must not be entirely discounted. Consequently some of the changes in relative level of sea and land may have been caused by phenomena entirely independent of the ice. (This type of earth movement may also have taken place in the Eastern Arctic, but is held to be more improbable than in the Western Arctic.)

LACK OF GLACIATION

Recent maps showing the extent of Wisconsin glaciation (Antevs, 1929; Flint *et al.*, 1945) show that the Arctic coast from the Mackenzie River westward to Bering Strait was unglaciated, except in the mountains; furthermore, it is well known that the Pleistocene glaciation of the rest of Alaska was confined to the mountains and the adjacent lowlands. Since there was no glaciation, one may question whether the land from the Mackenzie River westward to Bering Sea was seriously depressed by the ice; consequently, there may be no emerged strand lines and shell deposits in the region.

SEDIMENTATION IN MACKENZIE DELTA

The Mackenzie River and its tributaries drain a tremendous area of western Canada. While no figures are available on the silt load of this river, the amount must be enormous. A comparison can be made with the Mississippi River which transports an average of at least 2,000,000 tons of sediment to the Lower Delta every day (Russell, 1936: 162). It has been shown that this silt load has a definite effect on the Mississippi Delta, causing relatively rapid subsidence (Russell, 1936). Marine Pleistocene deposits which occur from the surface to a depth of about 100 feet in western Florida and Alabama and western Louisiana, are known at a depth of 2,700 feet in the delta below New Orleans (Richards, 1939: 306). While this apparent subsidence may be partly explained by other factors, the fact remains that there is reasonably good evidence of relatively rapid subsidence caused by the sediments brought down by the Mississippi River. A direct comparison between the Mississippi and Mackenzie Rivers is not possible for a number of reasons, for one, the Mississippi carries its silt load for twelve months of the year, while the Mackenzie is covered with ice for eight or nine months and can transport its silt load only during the short summer season. Nevertheless, it seems highly probable that some subsidence has taken place in the region of the Mackenzie Delta caused by the silts transported down that river. Subsidence may have counteracted any rise caused by isostatic adjustment from the load of the ice, and thus complicate the record of any postglacial marine shoreline.

RECENT OBSERVATIONS IN MACKENZIE DELTA

While time and facilities did not permit more than a brief reconnaissance during the trip of 1948, a few observations are of interest.

DELTA DEPOSITS

The islands in the Mackenzie Delta (e.g., Aklavik) are composed of silts apparently transported by the Mackenzie River during Recent times; no fossils were observed.

BED ROCK EAST OF DELTA

While no thorough survey was possible, no Pleistocene marine sands or clays were observed on top of the bed rock immediately east of the Delta. Nor did inquiry from various residents and travellers disclose the records of any shells in this region. The beach at Kittygazuit, opposite Richards Island contained numerous fossil shells, probably of Cretaceous age, and it is thought that these were washed from the hills inland from the coast. If Pleistocene fossils occurred anywhere in the vicinity, it is thought probable that some shells or other fossils would have been transported to the beaches, as is the case in many other parts of the Arctic (e.g., Hudson Bay).

FIG. 1. Air view of Mackenzie Delta northeast of Aklavik.

FOSSIL ICE

Fossil ice or ground ice consists of bodies of more or less clear ice in permanently frozen ground; some of this ice may be of great antiquity and date from the Late Pleistocene. A well exposed section of fossil ice was observed along the shore of the west branch of the Mackenzie River about five miles northeast of Kittygazuit, opposite Richards Island and near the junction of the river with the open Arctic Ocean. About 15 feet of ice was exposed overlaid with about 3 feet of sand. The thickness of the ice could not be determined since the base was not exposed. (See fig. 2, 3.)

Similar deposits of fossil ice are known along the shore of Herschel Island and have been reported from numerous places in Alaska, Siberia, and elsewhere in the Arctic (Leffingwell, 1919; Muller, 1947).

PINGOS

Leffingwell (1919) and Porsild (1938) have reported the existence of conical hills or mounds at various places in the Mackenzie Delta, as well as elsewhere in the Arctic. These are known as *pingos,* from the Eskimo word for conical hill. Several theories have been advanced to explain the origin of these pingos, but the one that seems most acceptable for those in the Mackenzie Delta is that of Porsild who believed that they were "formed by local upheaval due to expansion following the progressive downward freezing of a body or lens of water or semi-fluid mud or silt enclosed between bedrock and the frozen surface soil, much in the way in which the cork of a bottle is pushed up by the expansion of the water when freezing" (Porsild, 1938: 55). The largest pingo in the Mackenzie region is at Tuktuyakok (= Tuk Tuk) which according to Porsild is 134 feet in height.

While we did not see the pingo at Tuktuyakok, we did observe several smaller pingos while flying over the eastern part of the Delta between Aklavik and Kittygazuit. The "craters" of these pingos were filled with water.

Porsild has collected plant remains and freshwater mollusks from the layers of certain pingos. These apparently do not indicate any change in climate.

WEST OF THE DELTA

The hills some thirty-five miles west of Aklavik are apparently composed of Cretaceous rocks, as

FIGS. 2, 3. Ground ice, 5 miles northeast of Kittygazuit at mouth of east branch Mackenzie River.

demonstrated by marine fossils collected from the slope of Black Mountain near Husky Channel. Cretaceous (?) coal in low hills west of the west channel of the Mackenzie River about sixty miles north of Aklavik is mined for local use. In none of the places observed were the Cretaceous deposits overlaid by marine Pleistocene shells.

KAY POINT

The first conspicuous headland along the Arctic coast west of the Mackenzie Delta is at Kay Point which rises to an elevation of about 200 feet. O'Neill (1924) obtained Pleistocene marine fossils from the east side of Kay Point, about three miles from the tip; the shells occurred up to 25 feet above sea level, although the bluff at this point was 100 feet in height. Weather conditions made it impossible to land our plane at this point; however, I have had an opportunity to reexamine the material collected by O'Neill. The following species were present: *Natica* sp. (juv.); *Modiolaria* ? sp.; *Leda glacialis* Wood; *Pecten* ? sp.; *Astarte borealis* (Schumacker); *A. fabula* Reeve; *Macoma calcarea* (Gmelin); *Saxicava arctica*[2] Linné; *Dentalium*[2] sp.

HERSCHEL ISLAND

This island is located in the Arctic Ocean (Beaufort Sea) in latitude 69° 35′ N. and longitude 139° W., opposite the mouth of Firth River and about forty miles east of the International Boundary (Demarcation Point). The island is about ten miles east to west by five miles north to south (at its broadest), and is separated from the mainland by about two miles of shallow water. The bluffs along the shore range up to 50 feet in height; from this height the ground slopes upward to an elevation of about 558 feet (as indicated in Canadian Topographic Sheet 117 SW–SE).

Marine Pleistocene fossils were collected from the bluffs on the south side of the island about one-half mile west of the Royal Canadian Mounted Police barracks. (This is probably station 1926 of O'Neill.) Fossils were observed up to 100 feet above the sea, although the majority of the specimens were in the lower 50 feet. The following species have been identified: *Neptunea satura* (Martyn); *Buccinum tenue* Gray (frag.); *Natica cf. clausa* Broderip and Sowerby; *Trophon beringi* (Dall); *Tachyrynchus polaris* Beck; *Astarte borealis* Schumacker; *A. arctica* (Gray); *A. fabula* Reeve; *A. alaskensis* Dall (the most abundant species); *Cyrtodaria kurriana* Dunker.

O'Neill also reported fossils on the east side of Herschel Island up to 200 feet above the sea. The following were listed: *Natica* sp. (juv.); *Astarte borealis* Schumacker; *A. fabula* Reeve; *A. alaskensis* Dall.

ARCTIC COAST OF ALASKA

Schrader (1904) described the Gubik sand along the Arctic coast of Alaska and regarded it as of Pleistocene age; no exact fossil localities were

[2] Reported by O'Neill; not in material reexamined by present writer.

recorded, although he listed some "recent fossils from the Arctic coast between Colville River and Cape Lisburne."

Pleistocene marine mollusks have been reported at several places near Point Barrow, especially on Peard Bay (Leffingwell, 1919; Dall, 1921; Meek, 1923; Smith and Mertie, 1930), but their exact age is uncertain. In the recent glacial map of North America (Flint *et al.*, 1945) footnote No. 5 under Alaska states that the marine deposits along the Arctic coast of Alaska are probably of interglacial age. The species suggest deeper and more saline waters than indicated by the fossils from Herschel Island and Kay Point.

Mertie (McNeil, Mertie, and Pilsbry, 1943) in discussing the geological history of the Nome region assigns the fossiliferous deposits of the "second beach" to the interglacial stage prior to the Wisconsin glaciation. No postglacial marine shells were reported.

MIGRATION OF MAN TO NORTH AMERICA

Since there is no evidence of vertical movements of the land in northeastern Alaska during late Quaternary time, and since sea level was probably lowered 240 to 300 feet during the climax of the Wisconsin glacial stage, it would have been relatively easy for migrations to have taken place between Siberia and Alaska.

The migration route of early man from Asia to North America was apparently opened 20,000 to 15,000 years ago. This route may have extended across the then narrow Bering Strait and across unglaciated central Alaska to the Mackenzie River and then southward along that river and the eastern slopes of the Rockies (Antevs, 1935; Johnston, 1933).

POSTGLACIAL CLIMATE

Porsild (1938: 57) offers botanical evidence to suggest that the climate in the Mackenzie Delta is becoming colder and that the limit of the permafrost is extending downward. For example, peat deposits in the eastern part of the delta were found to contain the remains of the larch (*Potamotegon epihydrus* var. *nuttallii*) fifty miles north of its present northern limit. Furthermore, stumps of trees are reported on Richards Island, sixty to seventy miles north of the present tree line. It is highly probable, as suggested by Porsild, that these plant remains date from a warmer stage of post Wisconsin time (= Atlantic time ?).

On the other hand, Jenness (1949: 22) cites other evidence to suggest that there has been an amelioration of the climate in the northern hemisphere during the past half century. Jenness mentions particularly the work of Russian scientists who have claimed that the southern boundary of permafrost in the Archangel region of northern Russia has moved northward almost one hundred kilometers (sixty miles) during the past ninety-six years.

These two sets of data are not necessarily at variance. The climate may be colder now than during the postglacial temperature maximum (7,000–4,000 years ago), when the trees were living on Richards Island, but the present tendency may be toward a slight warming of the climate.

DISCUSSION AND SUMMARY

1. The evidence suggests that the northeastern Atlantic and eastern Arctic coast was depressed by the weight of the Wisconsin ice sheet. As sea level rose because of the melting ice, the water flooded the depressed land. Later, the land recovered from the effect of the load and slowly rose. Evidences of the former advances of the sea can be seen in the form of raised beaches and marine Pleistocene fossil deposits. These beaches range from a few feet above sea level in the vicinity of Boston to 898 feet in Hudson Strait, and to questionable elevations approximating 2,000 feet in northern Ellesmere Island.

2. There is no evidence of any postglacial marine beaches in the immediate vicinity of the Mackenzie Delta.

3. Marine Pleistocene shells have been found up to 25 feet above sea level at Kay Point and 100 feet on Herschel Island, although in both places non-fossiliferous silts occur to higher elevations (200 feet at Kay Point and over 500 feet on Herschel Island). The fauna indicates cold, shallow water of not too high salinity, probably not very different from conditions in the vicinity of Herschel Island today where the freshwater from the Mackenzie River has a decided diluting effect on the sea water.

4. No Pleistocene marine deposits were found on top of the bed rock (Cretaceous) either immediately east or west of the Mackenzie Delta.

5. The marine Pleistocene deposits along the Arctic coast of Alaska, especially in the vicinity of Point Barrow, as reported by earlier writers, are probably of interglacial rather than postglacial age.

6. Since the Arctic coast between the Mackenzie River and Bering Strait was unglaciated, it is probable that there was little depression and sub-

sequent uplift caused by the weight of the ice. Furthermore, it is believed that any postglacial rise of the land in the vicinity of the Delta would have been counteracted by subsidence caused by the sediments carried down to the Delta by the Mackenzie River.

7. O'Neill (1924) believed that the Delta region may have submerged 500 feet or more during late glacial time, and that consequently sea level may have extended inland as far as Great Slave Lake. However, as pointed out by Johnston (1933: 42). this may not have been so. There is no positive indication of the flooding of the Mackenzie Delta during late glacial time. The fossils at Kay Point and Herschel Island are west of the actual delta, and in fact have not been found higher than 100 feet above the sea, although the sediments extend to higher elevations. Air photographs of Great Bear and Great Slave Lakes show that the abandoned beaches are tilted toward the northeast, indicating that the Arctic coast region was more depressed than the Mackenzie Valley after the withdrawal of the ice sheet (Johnston, 1933: 42–43). Furthermore, the absence of any records of Pleistocene marine fossils from the shores of Great Bear and Great Slave Lakes, as well as along the lower Mackenzie River, affords negative evidence against a Pleistocene submergence of the Lower Mackenzie Valley.

8. There is evidence, cited by Porsild, that the climate was milder in parts of the Mackenzie Delta during one phase of the Pleistocene. On the other hand, evidence from other parts of the Arctic, cited by Jenness, suggests a slight amelioration of the climate during the past fifty years. These two statements are not necessarily at variance.

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STUDIES ON LETHAL E GENE IN NEUROSPORA TETRASPERMA, INCLUDING CHROMOSOME COUNTS ALSO IN RACES OF N. SITOPHILA[1]

B. O. DODGE, J. R. SINGLETON,[2] AND ANITA ROLNICK

The New York Botanical Garden

UNDER the title, "A new dominant lethal in Neurospora," Dodge (1939) reported on his work with a heterocaryotic race of *N. tetrasperma* in which the nuclei of one mating type carried the lethal *E* factor, the nuclei of the opposite mating type carrying the normal allele *e*. The fusion nucleus in every ascus would be heterozygous, *E/e*. This factor *E* had three distinct effects. (1) On Difco cornmeal agar about 99 per cent of the asci aborted without delimiting any spores. (2) Occasionally factor *E* was not dominant to cause abortion, therefore spores were cut out in that *E/e* ascus. However, instead of four large spores characteristic of *N. tetrasperma*, eight small spores were delimited. Such asci presented much the same appearance as that of normal asci of *N. sitophila*, and the spores were of the same size. (3) About 50 per cent of those spores from the eight-spored asci, *E/e*, died soon after germination.

It might appear that the only way by which the lethal heterocaryon could be maintained in culture would be by vegetative propagation. It was pointed out, however, that occasionally an ascus cuts out seven instead of eight spores. In that event one spore will be large and usually heterocaryotic for both lethal *E* and for mating type factors. (See pp. 45–46 regarding this fact.) Cultures obtained by germinating the large spores, being heterocaryotic, would give rise to perithecia with heterozygous, *E/e*, asci, which would delimit eight instead of four spores.

When lethal *I* (Dodge, 1934) was not effective in a particular ascus to cause abortion, that ascus cut out four spores. Why should lethal *E* under a similar situation manifest a strange and entirely different dominant effect? When *N. tetrasperma* is crossed with *N. sitophila* the rare F_1 ascus one obtains will delimit eight spores, though some of them will not mature. Even so, eight-sporedness is dominant to four-sporedness. How can this all be explained mechanically, genetically, and taxonomically? The answers seemed likely to involve questions of racial and specific chromosome differences, as well as other cytological features. It was with these and other questions in mind that this heterocaryotic race (*E-e*), has been kept in culture for the past ten years, awaiting the opportunity to undertake the work reported at this time.

ASCUS ABORTION PREVENTED

When the heterocaryon was first grown on Difco potato-dextrose agar instead of Difco cornmeal agar, ascus abortion was greatly reduced. When this work was resumed after an interval of about eight years, the Difco potato-dextrose agar now in stock was no longer very effective in preventing ascus abortion. After some experimenting with steeps made from varieties and grades of potatoes in the markets, it was found that a potato-dextrose medium, "PDS," made from a steep from new or half-grown potatoes gave us a medium on which there was practically no ascus abortion. In the cornmeal agar we have a medium most suitable for cytological studies of ascus abortion. The PDS medium is most valuable for genetical and cytological studies of eight-sporedness in *Neurospora tetrasperma*.

Physiologically ascus abortion behaves like the biochemically deficient mutants of *Neurospora*, occurring on cornmeal agar and the basal synthetic medium of Westergaard and Mitchell (1947), and the latter with supplements of yeast and malt extracts and casein hydrolyzate. On potato-steep dextrose agar, however, ascus abortion occurs in a very low percentage of asci. In general, the percentage of heterozygous, *E/e*, asci that abort without spore formation varies greatly depending on the kind of medium used. But, as pointed out above, an ascus that does not abort will cut out spores. Since 8-sporedness is dominant to four-

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[2] University of Missouri, Dept. Botany, Columbia, Mo.

sporedness in *E/e* asci, the percentage of 8-spored asci as compared with asci with fewer than eight spores is not affected by any medium thus far tried. In other words, the degree of dominance of the factor *E* over *e* to cause abortion depends on the kind of medium, but, the degree of dominance of *E* over *e* to regulate the number of spores delimited in these asci apparently is not changed by altering the medium.

HOMOCARYOTIC LETHAL *E* RACES GROWN IN CULTURE

The cells of the mycelium of a *Neurospora tetrasperma* strain, and the conidia borne in the culture, are usually multinucleate and heterocaryotic, not only for mating type factors *A, a,* but also for any other pair of factors associated with them. Our heterocaryon (*E-e*) will be either (*AE-ae*), or (*aE-Ae*), depending on the particular combination of these factors in the original ascospore. Starting with an ascospore, for example (*AE-ae*), there will be three kinds of conidia—*AE, ae* and (*AE-ae*). As shown by Dodge, Schmitt, and Appel (1945) the three kinds of conidia borne on a non-lethal heterocaryon can be isolated and grown separately in culture by using the well-known method of plating the conidia. Considering only the mating type factors, there will be three kinds of conidia (*A-a*), *A,* and *a,* because the two kinds of nuclei, *A* and *a,* which usually are closely associated (*A-a*), sometimes become separated either before, or at the time, the conidia are formed.

Judging from past experiences, it was assumed that it would be impossible to obtain a viable functional mycelium by germinating an ascospore that had at its origin only one haploid nucleus, and that one bearing the lethal *E* factor with no compensating normal, *e,* nucleus such as would be found in a large spore such as (*AE-ae*) or (*aE-Ae*). Germlings from small spores had always died. There was a chance, however, that, among the many multinucleate conidia formed on a heterocaryon such as (*AE-ae*), or (*aE-Ae*), there might be a few that happened to be homocaryotic for *E* and yet carry over some sort of cytoplasmic effect that would prevent the death of the germlings. In order to test this possibility, a number of heterocaryotic ascospores, some being (*AE-ae*) and others (*aE-Ae*), were grown singly in culture, each culture being given a distinguishing number. Conidia from a particular culture were plated out and allowed to germinate. Those that showed the slowest and most abnormal types of growth were transferred to PDS agar in tubes, after which they were incubated at 27° C. for a week or ten days. By that time many of the conidial transplant isolate cultures were producing perithecia. Such isolates must be exactly the same genotypically as the original heterocaryon and so were usually discarded without being given distinguishing numbers. Any culture that was to be kept was labeled. For example, culture "41C23" means that the original heterocaryon culture was from ascospore No. 41, and the C23 means that this particular culture originated from the twenty-third conidium isolated.

Other conidial isolates from the same plating soon began to grow rather vigorously. They had grown slowly at first because, being very small (chosen on that account), they had only two or three nuclei and a small amount of cytoplasm. When these isolates failed to produce perithecia they were assumed to represent the normal *e*—component of the heterocaryon. One or two from each plating were tested for mating type and kept.

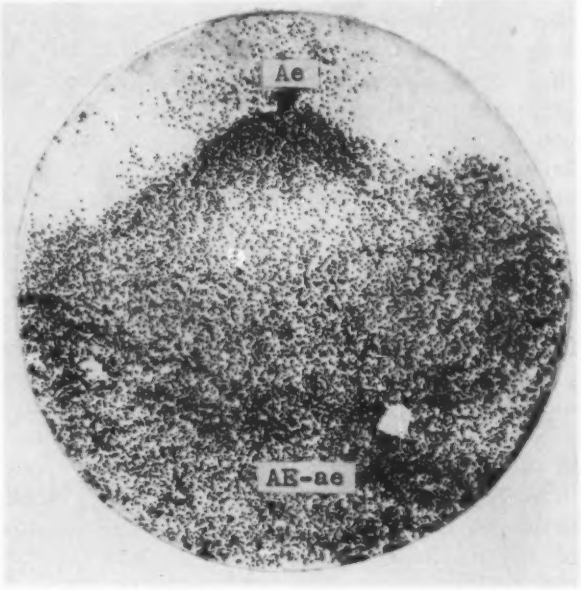

FIG. 1. *Neurospora tetrasperma.* Plate culture on PDS agar showing perithecial distribution: on the lower half, when the heterocaryon (*AE-ae*) fruits from self fertilization, asci will be 8-spored, a few may be 7-spored, one large and six small spores. Upper half shows pattern due to the migration of *ae* nuclei from the heterocaryon side, to join with the normal *Ae* nuclei to produce normal 4-spored asci. This shows how the mating types of a heterocaryon (*E-e*) can be proved without plating the conidia. See text for further discussion of the progeny of ascus No. 394, and figure 3 for pictures of the two kinds of asci, rosettes of 8-spored and rosettes of 4-spored asci.

This established the genotype of the normal component as *Ae* or *ae*, depending on the test result.

The third type of isolate cultures included those whose mycelia, not only grew very slowly, but after a week or so turned brownish or sordid, presenting a rather ugly appearance for a *Neurospora*. They were suspected of carrying *mostly* *E* nuclei. A number of them were very thoroughly tested not only for mating type but also for the presence or absence of lethal *E* nuclei. Most of them were a disappointment because they were finally proved to be very unevenly heterocaryotic. The presence of a few normal *e* nuclei had been temporarily masked owing to the greater number of *E* nuclei present. In the end at least eight of the isolates continued to remain self-sterile under every test, and therefore were considered to be pure for the lethal *E* factor. Six races were *AE* but only two races have continued to react as pure *aE*. The distinguishing numbers of the *AE* races are as follows: 3C3; 31C6; 71C10; 76C7; 86C7 and 103C9. Only 28C3 and 41C3 are *aE*. These races are being kept under mineral oil and will be available so long as they remain viable. We now feel certain that these represent examples of a haploid organism, all of whose nuclei carry a gene originally considered to be a dominant lethal gene that can be grown in culture indefinitely, that is, without any compensating normal nuclei. In each case the normal component of the heterocaryon was recovered and tested so that the genotypes of both components of the original heterocaryon were determined by test and not by interpolation. All isolates *AE* reacted perfectly with tester *ae* to produce perithecia with eight-spored asci, just as did race 28C3 (*aE*), when grown with tester *Ae*.

Several of the lethal races were grown together in various combinations of opposite mating types to see if perithecia with asci would develop. None developed in the crosses made. The ultimate aim of this project is, of course, to find some way to obtain homozygous, *E/E*, asci. This might well mean that this temporary reversion of *N. tetrasperma* to a *sitophila* eight-spored type is one of the first steps on the road to making it, the reversal to eight-sporedness, permanent. When all the other factors concerned are discovered, the secret of how a four-spored species can arise from an eight-spored type will be disclosed. See page 46 for discussion on this point.

Figure 1 shows how one can determine the genotypes of a hetercaryon by growing it in plates, first opposite one tester, *Ae*, then with the other *ae* tester. For example, heterocaryon (*AE-ae*) grown opposite *Ae* produces (*a*) eight-spored asci by "self-fertilization," and then (*b*) fruit bodies with only four-spored asci by cross fertilization, in this case mostly on the opposite side of the plate. Against tester *ae* only eight-spored asci will be found. (See also fig. 3.)

CULTURES FROM UNINUCLEATE LETHAL *E* ASCOSPORES

In spite of all our tests to the contrary, it was still believed that one could never be certain that any particular conidial isolate from the heterocaryon (*E-e*) was devoid of all normal *e* nuclei. On this account it was decided to try again to obtain pure lethal *E* races by germinating ascospores from heterozygous, *E/e*, asci. The eight spores in such asci will all be uninucleate at their origin, and four of them will contain a single lethal *E* nucleus; the other four will be normal *e* spores. The lethal gene does not prevent germination but, on cornmeal agar, the germlings had always died, at

FIG. 2. The ascus walls do not show in the photographs A, B, and C.

A. Asci of *Neurospora sitophila*, 8-spored. × 350.

B. Normal, *e/e*, asci of *N. tetrasperma*, 4-spored. × 350.

C. Heterozygous lethal, *E/e*, asci of *N. tetrasperma* grown on new-potato-steep dextrose agar, 7-spored as well as 8-spored asci are shown. × 350.

D. Heterozygous *E/e* asci of *N. tetrasperma* grown on cornmeal agar; all asci in this rosette aborted without spore formation. × 350.

E. Homozygous lethal, *d/d*, asci of *N. tetrasperma*. All asci abort. × 350. (See Dodge, 1935.)

F. Heterozygous indurated lethal, *I/i*, asci of *N. tetrasperma;* spores were not formed but the entire ascus has become indurated and striated like the wall of an ascospore. When an ascus does not abort, four spores will usually be formed. × 350.

G. Two nuclei in prometaphase of the third division in an ascus of the cross 56.2 (protoperithecial, non-conidial) × f_1 36 (protoperithecial, non-conidial) *N. sitophila*, each nucleus has the haploid complement of 7 chromosomes. In the lower nucleus chromosome 1, the largest chromosome, lies at "12 o'clock"; chromosome 2, the second largest, at 3 o'clock. × 1,000. Photograph by Dr. C. A. Berger, from an aceto-orcein "squash" preparation by the junior author.

H. One nucleus in prometaphase of the second meiotic division in an ascus of the cross 56.2 (protoperithecial, non-conidial) × 56.6 (non-protoperithecial, non-conidial) *N. sitophila*. 7 dyads are visible. Aceto-orcein squash. × 1,700.

I. Nucleus at diakinesis in an ascus of the cross 356.3 (*AE*) × 343.2 (*ae*), *N. tetrasperma;* 7 bivalents are present. Chromosome 1, the largest, lies at 12 o'clock. Aceto-orcein squash. × 1,700.

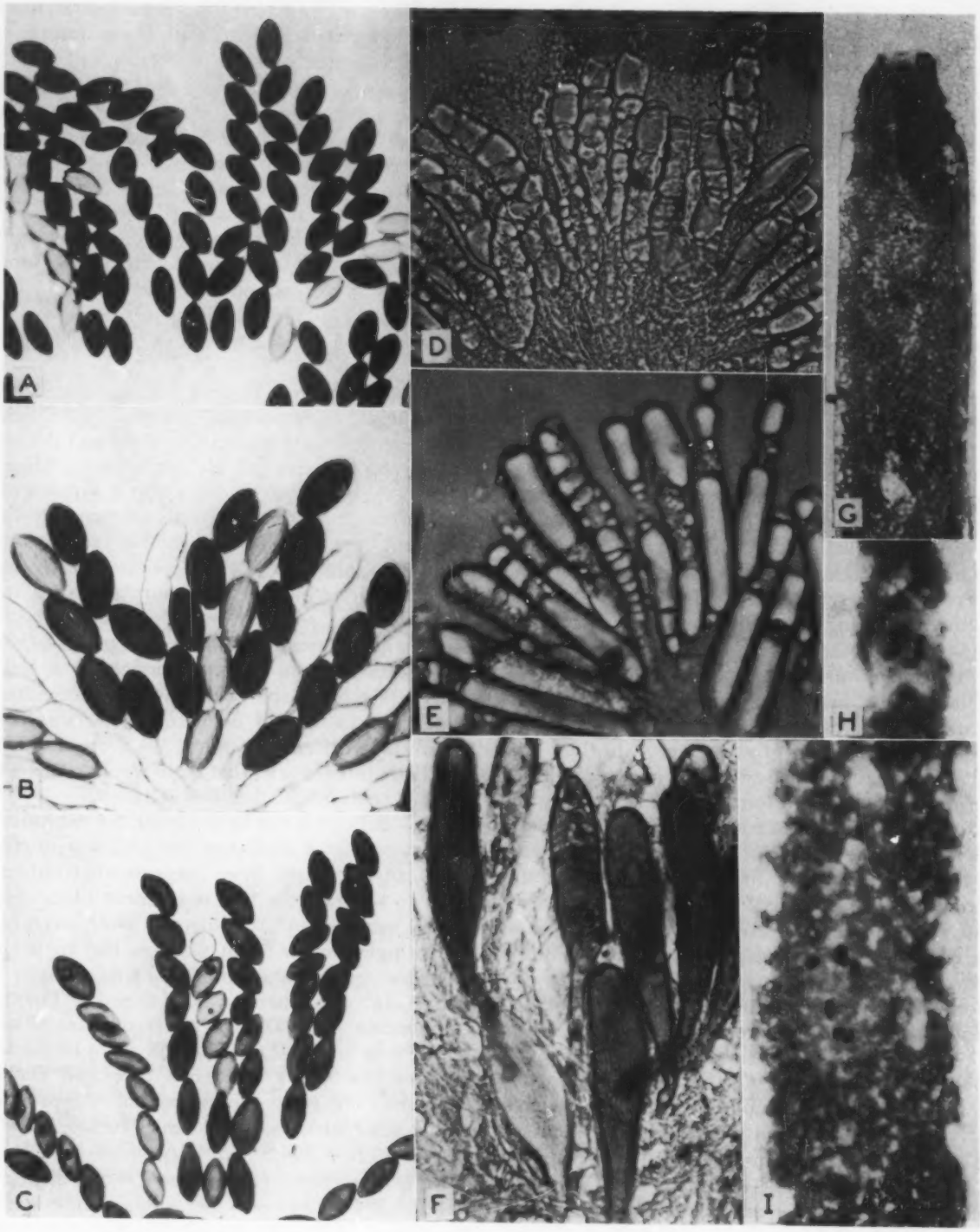

FIG. 2.

least so far as was learned at that time. The new-potato steep PDS medium had proved to be very effective to prevent abortion of asci and it had been found very good for culturing the conidial isolates described previously. It might also be effective to prevent the death of lethal *E* ascospore germlings. This would also provide a means of checking those conidial isolates such as 3C3 or 28C3. At that time no one could have known just how a pure homocaryotic *E* race should look and react in culture.

Using methods described many times elsewhere, the spores from either seven-spored or eight-spored asci were isolated without regard to their order of arrangement in the particular ascus chosen. They were transferred to plates of either cornmeal or PDS agar and given the heat-treatment. The spores that germinated were transferred to tubes of PDS. By repeating this heat-treatment the spores that did not respond to the first treatment sometimes did so following additional treatments. The tubes containing the germinated lethal *E* spores were kept at 27° C. for as long as two weeks. If no further growth occurred they were discarded. In the meantime the cultures growing normally were tested for mating type.

It was not until the spores from the thirty-second ascus had been isolated and given the heat-treatment that the lethal *E* germling, 332.6, was observed to be growing very slowly. The other three *E* spores had all germinated but the germlings died. The four normal *e* spores grew vigorously from the start and when tested were all found to be *Ae*. No. 332.6 was later proved to be *aE*. Its mycelium finally turned brown and sordid, flecked more or less with brownish growth patches. In appearance it was much like that of the conidial lethal *E* races such as No. 3C3. When grown with tester *Ae* many perithecia with eight-spored asci matured rather quickly. If one still doubted the purity of races Nos. 28C3, 86C7 and the others listed previously, there could be no doubt about No. 332.6. It, if not the others, can be regarded as the first purely homocaryotic race carrying only "dominant" lethal haploid nuclei to be grown alone in culture.

Ascus No. 341 was the next from which a germling carrying *E* grew to become a functional mycelium. This was a seven-spored ascus. Three of the spores proved to be normal *e*, one carried a lethal *E* nucleus, and the big spore was heterocaryotic. Two spores germinated but died. Without regard to their order in the ascus the six nuclei recovered were as follows: *Ae;* ?; *aE; Ae;* ?; (*aE-Ae*); *Ae*. All four normal nuclei were recovered and were *Ae* so that the two spores that died must have been *aE*.

The spores from some 120 asci in all were isolated without regard to their order in the ascus. None of the spores from certain asci germinated. In some cases the spores that did grow were not tested. The results reported below were obtained from the study of 63 asci. Eleven pure homocaryotic "lethal *E*" races were obtained directly from small (originally uninucleate) ascospores. They are all very similar to races No. 332.6 and 341.3 in their cultural characters, and all fruit perfectly with normal testers to give eight-spored asci.

It was interesting to find that, for a particular ascus, with two important exceptions noted below, all the normal *e* spores that grew were of the same mating type, either all *Ae*, or all *ae*. Thus in these asci there is no evidence that a simple cross-over had occurred between the centromeres and either *A* or *E*. Take for example asci Nos. 326 and 334. The four normal spores in each ascus were all *Ae*, while the four normal spores from ascus No. 331 were all *ae*. One ascus, No. 394, was highly interesting because it represents without question a cross-over ascus involving *A* and *E*. This was a seven-spored ascus. The large spore, 394.6 was proved to be (*AE-ae*), spores 394.4 and 394.7 were *Ae*, but spore 394.5 was *ae;* so we had two normal nuclei that were *Ae*, two that were *ae*, and one nucleus that was lethal *AE*.

How difficult it can be to isolate the two nuclear components of a heterocaryon (*AE-ae*) or (*aE-Ae*), can be realized when one sees what had to be done to separate the two component races represented in race 394.6. Altogether over three hundred single conidia from platings had to be isolated and grown singly in PDS tubes before the lethal *AE* component was obtained. The two components of 394.6 are referred to as 394.6_1, which is *ae*, and 394.6_2, which is *AE*. Depending on how one mates these five races, all derived from the same ascus mother-cell, all the products of the same meiosis, one obtains curious results. (1) The cross 394.5 × 394.4 gives 4-spored asci (fig. 3, B, right). (2) Either 394.5 × 394.6_2 or 394.5 × 394.6 gives only perithecia with 8-spored asci (fig. 3, A). (3) Mating 394.4 × 394.6 gives some perithecia with rosettes of four-spored asci when 394.4 mates with the 394.6_1 component of 394.6, but perithecia with eight-spored asci when the component 394.6_1 mates with the other com-

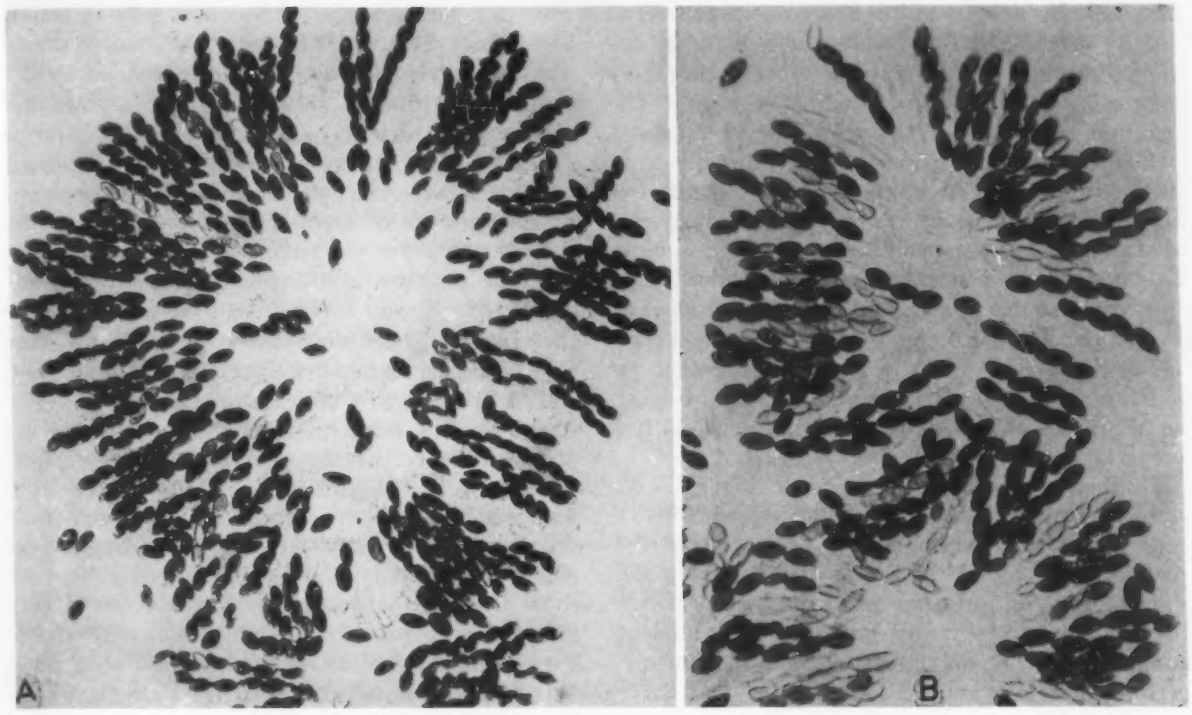

FIG. 3. Two rosettes of asci of *N. tetrasperma*, both at the same magnification. At the left asci heterozygous, *AE/ae*; at the right, asci homozygous for *e*, *Ae/ae*. (Compare with fig. 1.) If one mates the progeny from a heterozygous, *E/e* non-cross-over ascus, only one kind, 8-spored ascus rosettes, A, at the left, is possible. However, mating the progeny from a cross-over ascus such as No. 394, two kinds will be obtained: some rosettes will have all asci 4-spored and homozygous, *e/e*, as at B, right; other asci will be heterozygous, *E/e*, therefore 8-spored, as at A, left.

ponent, 394.6_2, of 394.6. Illustrating various possible matings giving the genotype in each case, we have:

394.4 (*Ae*) × 394.5 (*ae*) gives four-spored asci.

394.5 (*ae*) × 394.6_2 (*AE*) gives eight-spored asci.

394.6_1 (*ae*) × 394.6_2 (*AE*) gives eight-spored asci.

394.4 (*Ae*) × 394.6 (*AE-ae*) makes possible two kinds of matings:

Ae × *ae* gives four-spored asci; *AE* × *ae* gives eight-spored asci. Races 394.4, 394.7 and 394.6_2 are all mating type *A*, while 394.5 and 394.6_1 are both mating type *a*.

The four normal *e* nuclei recovered from each of eleven asci were all *Ae*, while the four from seven other asci were all *ae*. The three normal *e* nuclei recovered from each of five asci were *Ae*, and the three from each of twelve asci were all *ae*. Thus there is no evidence of a cross-over between *A* and *E* and the centromeres in thirty-five asci. This indicates that segregation of the mating type and lethal *E* factors had probably been at the first meiotic division, and that the genes *A* and *E* are not linked.

Only two of the four normal *e* nuclei from each of fourteen asci were recovered; in ten asci they were both *Ae*, and both were *ae* in four asci. So far as negative evidence goes, no cross-over was indicated where only two nuclei were recovered from an ascus, but there could have been one. A third nucleus *e* would have made the evidence positive one way or the other. Only one nucleus was recovered from each of twelve asci. It was *Ae* in four asci and *ae* in eight asci.

Disregarding this negative evidence there were, then, clearly two cross-over asci out of the thirty-seven asci scored, Nos. 394 and 353. Additional evidence will be presented later to support the view that mating type *A* and lethal *E* genes are both very close to their centromeres so that cross-over percentages between the two loci and their respective centromeres are of a low order.

All told the genotype of each of 170 normal *e* nuclei recovered was determined, 90 being *Ae*

and 80 being *ae*. In addition five nuclei recovered singly were *AE* and six were *aE*. Of the *E* nuclei recovered from the large heterocaryotic (*E-e*) spores, seven were *AE* and eight were *aE*. The parental mating was *AE* × *ae*.

The eleven lethal *E* races derived directly from ascospores originally uninucleate, and grown on PDS agar had the same morphological and cultural characters as those derived from single conidia as stated above. Since they look alike and act alike in every test made we know that it is possible to isolate the lethal *E* component of a heterocaryon (*E-e*) by plating out the conidia. Whether or not heterocaryons can be synthesized by growing an *aE* race with an *Ae*, or an *AE* with an *ae* has yet to be determined.

In their paper on heterocaryotic vigor in *N. tetrasperma*, Dodge, Schmitt, and Appel (1945) reported that no cross-overs involving either the mating type factor *A* or the recessive lethal gene, *d*, were found in forty-five asci from each of which they had recovered, and tested, the eight nuclear components. The unpublished part of their manuscript states that one ascus, No. 50, may have represented a cross-over because there seemed, doubtfully, to have been four kinds of nuclei (*Ad, aD, AD, ad*) represented. Their results as published forced them to conclude that perhaps their tests for the recessive lethal *d* may have been inadequate in a few cases, otherwise it would be necessary to interpret their results "as showing that these genes are so near the centromeres that crossing-over at those points is very rare." In view of the fact that mating type *A*, lethal *E* and the recessive lethal *d* genes are all near their centromeres and on separate chromosomes, it might be worth while to locate dominant lethal *I* on its chromosome and to try to grow lethal *I* races independently in culture.

SPORES IN ASCI DISSECTED OUT IN ORDER

Earlier work on the genetics of *N. sitophila* and *N. crassa* where the eight spores were isolated in order from an ascus, had proved informative. A second division segregation for certain factors was manifested by either a two and two (i.e., *AAaaAAaa*) or a two-four-two (i.e., *AAaaaaAA*) arrangement of the spores isolated. Second-division segregation percentages are now regularly accepted as measures, with certain corrections, of cross-over percentages between the centromere and the locus in those two species. However, as Lindegren (1933) clearly explained: "To express the distance between the spindle-fiber attachment and the locus of a gene in standard crossover units, the percentage of second-division segregation asci is merely divided by 2."

The dissection of spores at random from the eight-spored heterozygous, *E/e*, asci of *N. tetrasperma* gave the very gratifying results reported above. Nevertheless there remained the question of the genetics of eight-sporedness as related to the distribution of the normal *e*, lethal *E* and mating type *A* genes in individual asci, both in non-cross-over as well as in cross-over asci. It was therefore very desirable to do further work, this time culturing ascospores dissected in order. The results are reported under two sets.

Set I. The same methods as before were employed except that the perithecia crushed were washed with a solution of Clorox diluted 1 : 1 with water to kill the conidia. The spores from seventy-seven asci derived from the mating, *aE* × *Ae*, were isolated in order, notes being kept to show where two spores may have become interchanged in the process, and which asci were seven-spored and the location of the big spore.

It is rather remarkable that on PDS agar medium the spores should be very often so evenly lined up in an ascus. Dodge (1939: fig. 3) shows how in some crushed mounts the spores are often irregularly distichous so that one must expect to find asci in which the order of the spores, as now manifested clearly, may have been changed somewhat through side-slipping.

At least one normal *e* spore from each of fifty-four asci germinated to produce a normal mycelium. All four normal *e* nuclei from each of eleven asci were *Ae*, and the four from each of three asci were *ae*. Three nuclei recovered from each of seven asci were *Ae*, and the three from six asci were *ae*. Here, there were twenty-seven asci adequately proved to be non-cross-over asci. The only two nuclei recovered from each of nine asci were both *Ae* and the two from each of three asci were both *ae*. No cross-over from these twelve asci was manifested, though there could have been one not detectable when the two recovered were both *Ae* or both *ae*. Disregarding such asci we have, in twenty-seven asci scored, further proof of the nearness of the factors *A* and *E* to their centromeres. Including the thirty-seven asci dissected at random we have only two cross-over asci against sixty-two non-cross-over asci.

There were twelve asci from which the only *e* spore recovered was in each case *Ae* and the one from each of three asci was *ae*.

Thirteen lethal *E* races that grew very well and functioned perfectly in sexual reproduction were obtained. Five were *AE* and eight were *aE*. Of the six "big" spores that grew, two were (*AE-ae*) and four were (*aE-Ae*).

Set II. In addition to the results recorded in Set I, further work on this same question is reported here separately. The parental races crossed were also *aE* × *Ae*. As before a number of asci were not usable because none of the spores isolated grew. The four normal *e* nuclei recovered were all *Ae* in two asci, and all *ae* in five asci; all three nuclei typed in each of three asci were *Ae*, and all three in each of seven asci were *ae;* the two normal *e* nuclei recovered from each of eight asci were both *Ae*, and both were *ae* in each of ten asci; one each in twelve asci was *Ae*, but it was *ae* in each of twelve other asci. In all, seventeen asci were proved to be non-cross-over asci, while the eighteen asci from each of which only two normal *e* nuclei were recovered, furnished no evidence for a cross-over. On the other hand, ascus No. 48 with two of its recovered nuclei *Ae* and one nucleus *ae*, and ascus No. 206 with one nucleus typed as *Ae* and another nucleus as *ae*, do represent cross-over asci, even though one would have welcomed the recovery of two or three additional nuclei from each of these asci to make the evidence more convincing.

The eighteen asci from each of which only two nuclei were recovered also favor the view that cross-overs involving genes *A* and *E* are rather rare. These genes lie close to the centromeres on two different chromosomes.

SEGREGATION PATTERNS OF SPORES IN ASCI

Since in *N. sitophila* and *N. crassa* simple non-cross-over asci always show a "four and four" first division segregation, we might expect to find this same type operating in the eight-spored *N. tetrasperma* race because the results of the study of the thirty-five asci dissected at random had shown that cross-overs were rather rare for the genes *E* and *A*. It was interesting, therefore, to find that the "two and two" and "two-four-two" arrangements of spores in individual asci generally prevailed here practically to the exclusion of the "four-four" distribution type. Only one such arrangement was found in the first set dissected in order. Ascus SD. 284 as recorded showed that spores Nos. 1, 2 and 4 were *ae;* this would indicate a "four and four" distribution arrangement such as *ae, ae, ae, ae, AE, AE, AE, AE,* unless interchange involving certain spores had occurred. In all other asci two of the four spores of one mating type, *Ae*, for example, were in one end of the ascus and the other two were in the opposite end. Or, if all eight spores could have been typed, the arrangement would be: *Ae, Ae, aE, aE, Ae, Ae, aE, aE.*

The following examples make this point clear:

Ascus No. 292: ?, *aE, Ae, Ae,* ? ? *Ae, Ae.*
Ascus No. 345: *ae, ae,* ?, ? *ae, ae,* ? ?
Ascus No. 321: *Ae,* (*Ae-aE*), ?, *Ae,* ?, *aE,* ?
Ascus No. 325: *ae,* (*ae-AE*), ?, *AE, AE, ae, ae.*
Ascus No. 290: ?, *aE, Ae,* ?, *Ae, Ae, aE,* ?

Since it is possible to interpolate correctly the genotype of the spores that did not grow further, we would have, enclosing the missing spores in brackets [ ]:

Ascus No. 292: [*aE*], *aE, Ae, Ae,* [*aE*], [*aE*], *Ae, Ae.*
Ascus No. 345: *ae, ae,* [*AE*], [*AE*], *ae, ae,* [*AE*], [*AE*].
Ascus No. 321: *Ae,* (*Ae-aE*), [*aE*], *Ae,* [*Ae*], *aE,* [*aE*].
Ascus No. 325: *ae,* (*ae-AE*), [*AE*], *AE, AE, ae, ae.*
Ascus No. 290: [*aE*], *aE, Ae,* [*Ae*], *Ae, Ae, aE,* [*aE*].

Asci Nos. 292, 345, and 321 were of the "two and two" type, yet they were non-cross-overs; asci 325 and 290 were of the "two-four-two" type and they were also non-cross-overs. It happened that the second spore in each seven-spored ascus here was large and heterocaryotic. An explanation of this seemingly anomalous situation regarding spore size and spore arrangement in these seven- and eight-spored asci, based on spindle orientations and movements, especially at telephase of the second meiotic division, will be proposed later by the junior author (Singleton) who is making a further cytological study in this connection.

In Set I no attempt was made to fix the positions of the *E* spores in an ascus unless they germinated and grew to produce functional mycelia. In Set II, however, any spore that was seen plainly to germinate, even though the germling died later, was scored as lethal *E*. Of course its mating type pattern could not be fixed except by interpolation unless it lived.

There were in all twenty-five asci in Set II that were seen to represent either the "two and two" or the "two-four-two" patterns for both *A* and *E* genes. Four or five other asci showed doubtfully the same pattern for both genes. For *N. sitophila* this would mean twenty-five or thirty cross-over asci where, as a matter of fact, only two such asci were found,—Nos. 48 and 206. Interpreting liberally, four other asci might be recorded as ex-examples of a "four and four" arrangement for *E* and the same pattern for *A*. Further work being done will certainly clear up this phase of the question.

In normal *N. tetrasperma* where the two adjacent spindles at telophase of the third division "straddle" so that two nuclei, as reorganized, are thereby too far apart to cooperate in spore delimitation, two small spores will be cut out. Dodge (1942) illustrates this principle in his figure 2. They will usually be of opposite mating type. Curiously enough in our heterozygous, *E/e*, ascus where seven spores are delimited (see our figure 6 and the legend), it is, in effect, the *approach* of the reorganizing nuclei at one end of the spindles, while the nuclei at the opposite ends of the same spindles might even keep their positions, that must operate to cut out a large spore which is generally heterocaryotic for mating type as well as lethal *E*. This is the logical explanation for asci like Nos. 321 and 325 mentioned above. The high percentage of the "two and two" or two-four-two "phenotypic" second division segregation for *A* and *E* favors the occasional formation of these big heterocaryotic (*E-e*) spores in seven-spored asci. In our figure 5, E, F, the two spindles of a pair do not "straddle"; therefore eight spores will be delimited. It would be interesting to study genetically the occasional seven-spored asci in *N. sitophila* and *N. crassa*.

TAXONOMIC CONSIDERATIONS

The genus *Neurospora* was proposed by Shear and Dodge (1927) to include those species of the Sphaeriales having dark-colored striated ascospores and orange-colored monilioid conidia borne on dichotomously branched sporophores. They chose the well-known bakery mold, often called the red bread mold, as the type species. This mold, they said, was probably first mentioned in scientific literature in 1843 when Léviellé referred to it under the name *Oidium aurantiacum* which he described and illustrated. Montagne, later in the same year, also described the fungus under the name *Penicillium sitophilum*. Shear and Dodge discussed the systematic relations of these red bread molds and compared them with species of other genera having similar ascocarps. They found no evidence suggesting that species of *Neurospora* arose directly from any known ascomycete in particular. They said: "Included in *Sordaria* and *Podospora* are certain species which show many points of relationship with the fungi treated here." *Neurospora sitophila* could very well have been the ancestral type from which the other species arose.

Three species have eight-spored asci. They differ in the size of their ascocarps and in the dimensions of their asci and ascospores. Three other species have four-spored asci. *N. toroi* and *N. tetrasperma* are distinguished by the fact that they do not hybridize, at least readily. No doubt the first eight-spored species gave rise to the other two, but this does not necessarily mean that the first of the four-spored type gave rise to the other two. Regardless of how new species arise in nature, it is certain that the first four-spored species of *Neurospora* arose from an eight-spored type and not the other way around. We shall, more or less incidentally, point to some facts which suggest that *N. tetrasperma* may have arisen rather suddenly from an eight-spored *N. sitophila* type. These two species differ markedly not only in the features usually considered by taxonomists, but also cytologically and physiologically. On the other hand what appear at first to be wide differences, might be accounted for very simply. We may consider first their cytological differences, especially nuclear behavior and spindle orientations at stages between nuclear fusion and spore delimitations in the ascus.

CYTOLOGICAL CONSIDERATIONS

In the four-spored species, *N. tetrasperma*, as figured by Dodge (1927) and confirmed by Colson (1934), the first or reduction division spindle is longitudinal in the ascus so that the two daughter nuclei may come to rest one somewhat above the other. At metaphase of the second division the spindles are often found in tandem one well above the other. At telophase the spindles were obliquely placed and parallel, simulating the so-called conjugate divisions in the basal cells in aeciospore chains in the rust fungi and in clamp connections and crosiers in the higher basidiomycetes and ascomycetes.

No one seems to have found, as yet, the spindles at metaphase of the second division overlapping as if in conjugate division. Regardless of how the

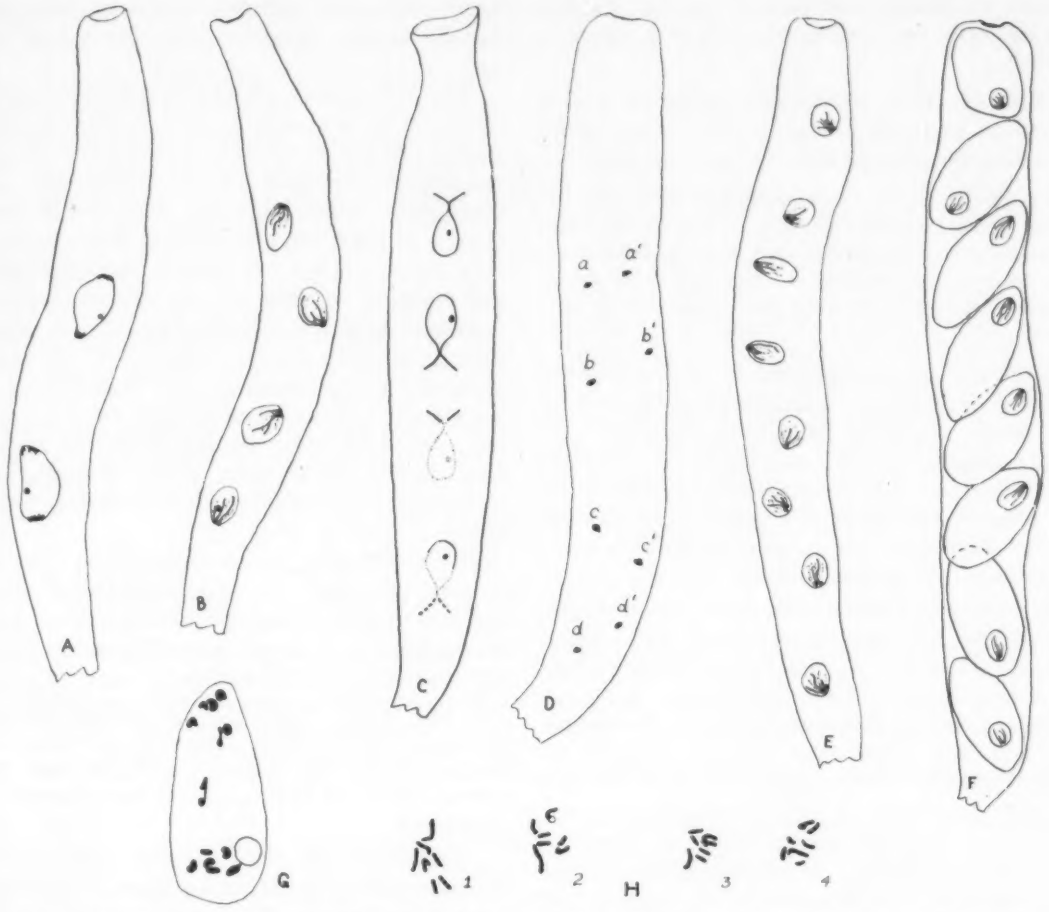

FIG. 4. *N. sitophila.* Semi-diagrammatic camera-lucida drawings from aceto-orcein squashes, except as indicated.

A. Telophase of second meiotic division, showing sister nuclei orientations of chromosome. × 690.
B. Interphase following second meiotic division, showing sister nuclei orientations and relic polar orientations of chromosomes. × 690.
C. Same stage as B adapted from Wilcox, showing "horns" (centrioles). Dotted lines not shown in her illustrations.
D. Telophase of third division (equational); sister chromosome groups indicated by *a-a'*, *b-b'*, etc. × 690.
E. Interphase following third division, after re-orientation of nuclei; relic orientations of chromosomes. × 690.
F. Ascus with spores uninucleate; relic orientation of chromosomes. × 690.
G. Nucleus at anaphase in the first meiotic division in ascus of cross 56.2 (protoperithecial), × 56.6 (non-protoperithecial); 6 dyads at each pole, lagging bivalent on the plate; nucleolus passed entirely to one pole. × 1,950.
H. Four figures from ascus 56.2 × 56.6 at metaphase of the third division; 7 chromosomes in each figure. × 1,950.

spindles at various stages of second division are oriented, the four reorganized nuclei finally come to rest in pairs in the sporeplasm, one pair of non-sister nuclei in the upper, the other in the lower part of the ascus. At this stage the nuclei are pear-shaped, their beaks being marked by forked appendages which are probably central bodies or centrioles. The orientation of the beaks is very characteristic in that the beaks of the pairs point in opposite directions or away from each other (Dodge, 1927: fig. I). In seeking to account for the positions of the four resting nuclei following a spindle orientation such as shown in his plate 1. D., he says:

> Since they [the spindles] do not lie on the same axis of the ascus a shifting of the position of the reorganizing nuclei, still connected by fibres, must take place, so that a pair of non-sister nuclei will come to lie in each end of the ascus. This is similar to the shifting of pairs of nuclei in the promycelium

described by Dodge and Gaiser (5) for *Caeoma nitens* except in the reverse order and with the opposite effect.

In a later paper on spindle orientation in *Gelasinospora tetrasperma* Dodge (1937: 878) under the heading "second nuclear division" we find:

> Although we have 32 different [sections of] asci showing second nuclear divisions, and in all cases one spindle is well above the other and separated from it by a considerable distance, we are still uncertain as to just how it is that the four nuclei reach their final positions.

While the tandem arrangement of second division spindles in *Neurospora tetrasperma* as figured by Dodge show nuclei at metaphase, the only asci with overlapping spindles are shown at telophase of this division. In *Gelasinospora*, however, he does show in his figure 10, plate 32, a pair of spindles which are still in tandem at telophase.

According to Wilcox (1928: pl. 2) in *N. sitophila* the two reorganized nuclei in an ascus, following the first meiotic division, move apart so that the spindles of the second division are oriented lengthwise in the narrow ascus. The four resulting nuclei are distributed in a row along the ascus. At this stage they are pear-shaped or beaked, the beaks being tipped by horn-like appendages. The beaks of the two upper-sister nuclei extend in opposite directions as do those of the lower-sister nuclei. A reexamination of her original preparations shows that she figured all these stages rather accurately. (See also our figure 4.)

If the ascus of *N. sitophila* were to become either somewhat thicker or shorter, then the telophase spindles of the second division could very well elongate to become overlapped "conjugately" just as they do at this stage in *N. tetrasperma*. In this way this striking cytological difference in these two species may be accounted for very easily.

When asci of normal *N. tetrasperma* delimit five spores, as noted previously, the two small spores often lie in the narrow lower end of the ascus. If, for some basically genetic reason, the whole ascus should become narrowed or streamlined, then it would not be surprising to find eight, instead of four, spores regularly cut out.

NUCLEAR AND SPINDLE ORIENTATIONS IN HETEROZYGOUS, *E/e*, ASCI OF *N. TETRASPERMA*

Since the asci and spores of the eight-spored asci of this strain are morphologically so much like those of *N. sitophila* we might expect that their cytological features would be very similar. It will be seen, however, that this is true only in part.

The first meiotic division spindle is longitudinal in the ascus the same as it is in the normal, *e/e*, ascus. Spindles of the second division are in tandem at the time of metaphase, but spindle elongation causes the two central chromosome groups to approach and, at least occasionally, pass each other. It has not been definitely determined cytologically whether or not this elongation and overlapping is a constant affair, and to what degree it actually occurs. However, the orientations of the four nuclei at interphase following the second division are usually like those in normal, *e/e*, asci, leading to the inference that the nuclei have passed each other, either through spindle elongation or some other kind of nuclear readjustment.

A characteristic orientation of the four nuclei at early prophase and prometaphase of the third division indicates that in the majority of asci the two upper nuclei are at about the same level, lying across the ascus from each other (fig. 5, D). The two lower nuclei are often staggered and so do not lie at the same level. In normal, *e/e*, asci the picture at this stage is much the same except perhaps that the two upper nuclei are more often staggered.

Spindles of the third division (fig. 5, F) lie obliquely across the ascus but there is considerably more space between the second and third spindles than between the two spindles of the pairs. This orientation picture may be described as a compromise between that seen in normal *N. tetrasperma* and *N. sitophila* at this stage.

The eight nuclei resulting from the third division remain in their telophase positions, and, usually, each cuts out a spore. Thus, the eight spores are at first arranged in two rows of four (fig. 5, G) at the time they are delimited. Later they shift their positions in most asci so that they become alined in a single row of eight spores (fig. 5, H). The genetic evidence previously given shows very clearly that in this readjustment the sister spores usually keep together as they line up. In some asci under certain culture conditions, all or part of the distichous arrangement of spores persists, as was clearly shown by Dodge (1939: fig. 3).

On cornmeal agar on which medium asci usually abort, no difference has as yet been detected in the cytological picture until after the third division. Instead of cutting out spores, the eight

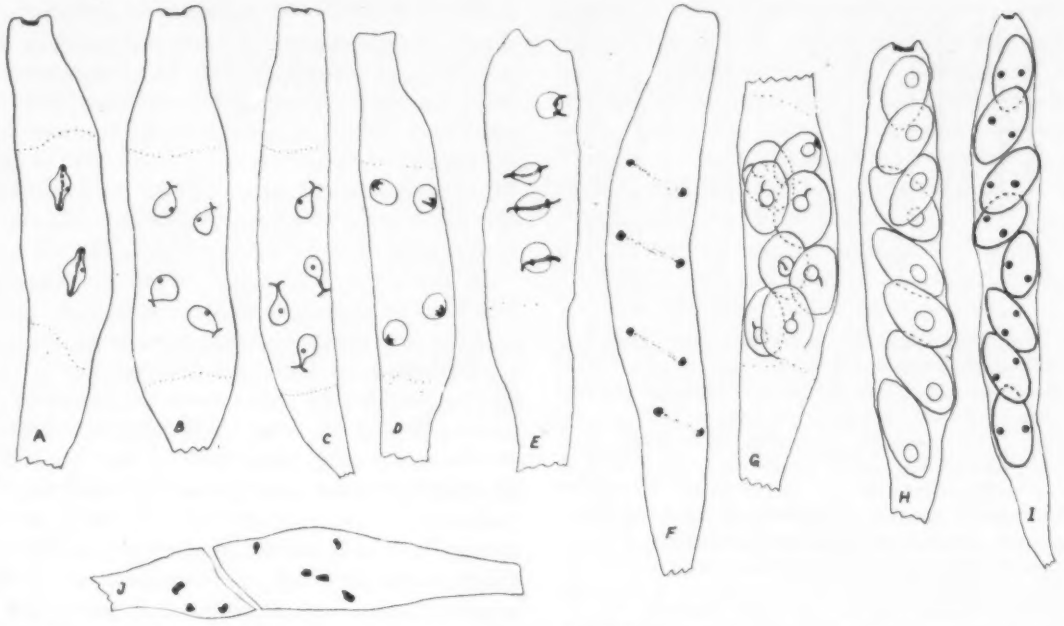

FIG. 5. Cytology of lethal ascus, *E/e*, in *N. tetrasperma*. Semi-diagrammatic camera-lucida drawings from Flemming's medium-Delafield hematoxylin sections, except as indicated.

A. Telophase of second meiotic division; spindles not overlapped. × 960.
B, C. Two asci in interphase following second meiotic division. B shows typical spacing of nuclei, C is more rare. Note centrioles and nuclear orientations. × 960.
D. Late prophase of third division, showing relic polar orientation of chromosomes and typical spacing of nuclei in prophase. × 960.
E. Telophase of third division, showing centrioles and usual spacing of nuclei; spindles of uppermost nucleus at right angles to those of the other nuclei. × 960.
F. Telophase of the third division from aceto-orcein squash; spindles still visible. × 960.
G. Eight-spored ascus, showing centrioles and bi-seriate arrangement of young spores, sister spores are side by side in pairs. × 960.
H. Uninucleate spores after shifting into uniseriate arrangement. × 960.
I. Binucleate spores. × 690.
J. Aborting ascus from cornmeal agar culture; the eight nuclei from the third division have remained condensed and pycnotic. × 960.

nuclei remain pycnotic, condensed and are embedded in the strands of cytoplasm which remain (fig. 5, J).

CHROMOSOME COUNTS[3]

For the chromosome studies reported here both aceto-orcein squashes and paraffin sections were utilized. Material to be stained with aceto-orcein was first fixed by immersing blocks of agar bearing perithecia in 3:1 absolute alcohol-glacial acetic acid, and then kept in a refrigerator at 10° C. until needed. The usual methods for this type of staining were employed. Sharper chromosomal staining and better differentiation resulted when preparations were allowed to stand without a cover-slip for from three to five minutes. The results were further checked by a study of stained serial sections.

[3] See legends under figures 1, 4, and 5 for details not given in the text.

CHROMOSOMES IN NORMAL, *e/e*, AND IN HETEROZYGOUS, *E/e*, ASCI

Colson (1934) reported that the strains of *N. tetrasperma* she worked with had a haploid number of six chromosomes. All her counts were made on sectioned material. McClintock (1945) observed seven chromosomes in squash preparations studied in collaboration with E. A. Weaver. Cutter (1946), using the acetocarmine squash technique, reported six chromosomes.

In the course of the present study the aceto-orcein squashes were made of asci obtained from the crosses: *Un 2(Ae)* × 343.2(*ae*) for the normal homozygous, *e/e*, asci; and *Un 2(Ae)* × 365.7

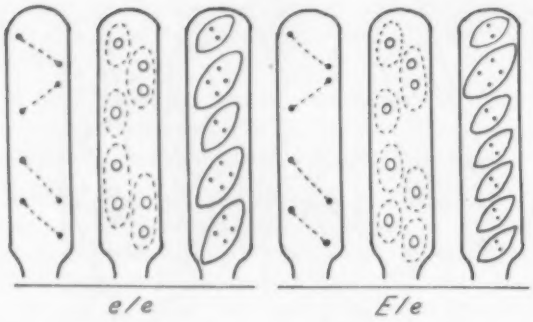

FIG. 6. Diagrams showing how convergent spindles at the third division in the normal (*e/e*) ascus of *N. tetrasperma* (the three asci at the left) and the heterozygous lethal (*E/e*) ascus (three asci at the right) could result in the formation of small, originally uninucleate spores in the *e/e* ascus and of large, originally binucleate spores in the *E/e* ascus. The ascus at the left of each group of three represents the situation at telophase of the third division; the next ascus to the right, at the time of spore delimitation, and the ascus to the right in each group, the final arrangement of spores at maturity.

(*aE*) and 343.2(*ae*) × 356.3(*AE*) for heterozygous, *E/e*, asci.

In all three crosses clear well-spread figures showed seven bivalents at diakinesis and metaphase of the first meiotic divisions; seven dyads at metaphase of the second meiotic division; and seven chromosomes at prometaphase and metaphase of the third (equational) division. Thus in the strains studied, the haploid number is seven. There is no difference in chromosome number associated with the lethal *E* factor in heterozygous asci.

CHROMOSOME NUMBERS IN STRAINS OF N. SITOPHILA

The difficulty one encounters when attempting to obtain fertile perithecia by crossing, interspecifically, races of *Neurospora*, has raised the question regarding possible differences in the number and morphology of their respective chromosomes.

For the study of this question as related to *N. sitophila*, a number of strains differing clearly as to certain genetic factors for the production of conidia and protoperithecia were available. In view of the recent publication of Hirsch (1947), it became of still further interest to determine whether or not these genetic characters involved differences in chromosome numbers. The factors with which we are concerned here have been designated as follows: *A, a* refer to mating type genes; *P*—produces protoperithecia; *p*—does not produce protoperithecia; *C*—produces conidia; *c*—does not produce conidia. Upper-and lower-case letters are used as a matter of convenience here, and not as a designation of dominance. Each pair of factors behaves as a single genetic locus. The following are the races studied cytologically: 56.2 is *aPc;* 56.6 is *Apc;* $f_1$36 is *APc;* and $f_1$22 is *apC*.

In the cross 56.2 × $f_1$36 seven bivalents were observed at diakinesis and metaphase of the first division and seven chromosomes were present at prometaphase of the third division (in three out of the four nuclei in an ascus). In asci of the cross $f_1$36 × $f_1$22, seven bivalents were observed at diakinesis and metaphase of the first meiotic division. No asci were found in which the chromosomes in more than two of the four third division nuclei could be counted with certainty; in those nuclei in which the chromosomes could be counted, seven chromosomes were observed. In addition, it could be determined in several pachytene nuclei that all chromosome strands were paired which would not be expected if a univalent were present.

In asci of the cross 56.2 × 56.6, seven bivalents were counted at diakinesis and metaphase of the first meiotic division. In one ascus at metaphase of the third division, seven chromosomes could be observed in all four nuclei; in one other ascus seven chromosomes were counted in three of the four nuclei at prometaphase of the third division (fig. 4, H).

Thus it may be concluded that all four races of *N. sitophila* studied have seven chromosomes as the haploid number. The cross 56.2 × $f_1$36 shows that no difference in chromosome number is associated with mating type; therefore, the cross 56.2 × 56.6 shows that no difference in chromosome number is associated with the ability, or lack of ability, to produce protoperithecia or receptive structures. From this, it follows from the cross $f_1$36 × $f_1$22 that no difference in chromosome number is associated with the ability or lack of ability to produce macroconidia. Races of *Neurospora* which are unable to produce protoperithecia might be considered analogous to the "male" races of *Hypomyces* reported by Hansen and Snyder (1946). In *N. sitophila*, this deficiency is not associated with the presence or absence of whole chromosomes as reported in *Hypomyces* by Hirsch (1947).

SUMMARY AND CONCLUSIONS

Ascus abortion in *Neurospora tetrasperma*, due to the presence of the dominant lethal in heterozygous, *E/e*, asci, was prevented by growing the heterocaryon on a new-potato-steep dextrose medium (PDS). Instead of aborting without spore-formation, the ascus cuts out eight (sometimes seven) spores instead of four. Such asci cannot be distinguished readily from those of *N. sitophila*.

The lethal *E* component nucleus of each of several different heterocaryons (*E-e*) was isolated from the normal, *e*, component nucleus, by plating out the monilioid conidia. In this way eight pure homocaryotic, *E*, races were isolated, grown on PDS agar and mated with normal tester races of this species to produce asci with eight instead of four spores.

By growing germlings from lethal *E* ascospores from eight-spored asci, *E/e*, on PDS agar, thirty-one pure lethal *E* races were obtained. Both mating types were represented. The races resembled the conidial *E* isolates culturally and morphologically, and produced eight-spored (sometimes seven) asci when mated with their normal testers, *Ae* or *ae*, according to the mating type of the lethal cultures being tested. This proved clearly that one can, not only isolate the two components of the heterocaryon (*E-e*) by plating the conidia, but one can obtain pure *E* races by growing small ascospores in culture.

Cultures were obtained by isolating the eight (or seven in seven-spored asci) spores from individual asci, either at random or in the order of their alinement. Four of the eight spores are always normal, *e*, representing wild type *N. tetrasperma*, the other four must be pure for the mutant *E* gene.

Attempts to obtain perithecia with homozygous, *E/E*, asci by mating mutant *E* races have as yet been unsuccessful.

Crossing-over between either mating type and lethal *E* genes and their centromeres is rather rare, so that as a rule all four normal *e* nuclei in an ascus will be of the same mating type, either *Ae* or *ae*, while the other four spores from that ascus will be either *aE* or *AE* respectively. Mating type and lethal *E* genes are not linked.

Results obtained by culturing from spores isolated in order prove that, while segregations of mating type *A* and lethal *E* factors, with few exceptions, occur at the first meiotic division, the spores do not alternate "four and four" in the ascus as would be the picture for non-cross-over asci of *N. sitophila* and *N. crassa*. Instead, the distribution is, as a rule, either the "two and two" or the "two-four-two" type. Phenotypic second division segregation here is not at all synonymous with genotypic second division segregation. Phenotypic first division segregation of mating type *A* and lethal *E* factors is rare, as are cross-overs involving these factors.

Chromosome counts were made of normal, *e*, as well as lethal *E* races of *N. tetrasperma*. The haploid number is seven chromosomes.

Counts were made of four genotypically different races of *N. sitophila:* protoperithecial, non-protoperithecial, conidial, and non-conidial, both mating types being represented. In all the races studied the haploid number of chromosomes is seven.

These studies show, further, that the difficulty of obtaining fertile perithecia when one attempts to cross *N. tetrasperma* and *N. sitophila* is not due to any difference in the numbers of their chromosomes, although the genetic position of the mating type locus, as well as other genes, in these species relative to their centromeres respectively, might be a factor.

Eight-sporedness is dominant to four-sporedness within *N. tetrasperma* itself, that is, *E* is dominant to *e*. When *N. tetrasperma* is crossed with *N. sitophila*, eight-sporedness is again dominant. These two species certainly have other important differences in addition to the numbers of spores in their asci. Nevertheless this temporary reversion of *N. tetrasperma* to an eight-spored *N. sitophila* type may very well indicate in reverse one of the steps leading to the origin of the former from the latter species.

A fourth distinct effect of lethal *E* was discovered when it was learned how to grow lethal *E* races independently in culture. Their growth rate is well below that of normal races of this species. They differ markedly in morphological, color, and other cultural characteristics, from those of normal races. The heterocaryon (*E-e*) races, however, grow and fruit much like normal races, but this need not mean that this illustrates a case of heterocaryotic vigor. The lethal may be only passive in some respects, while the normal *e* associate controls most of the developmental processes up to the time when asci are differentiating. Although this lethal *E* does have four very distinct effects as noted, so far as yet learned, it segregates as a single unit.

Germling lethality behaves, in part at least, like a biochemical deficiency, inasmuch as the

percentage of *E* germlings that grow can be increased from 9.5 per cent (on cornmeal or potato-dextrose agar) to 45.9 per cent (on Westergaard and Mitchell basal supplemented with yeast and malt extracts and casein hydrolyzate). The characteristic growth habit of *E* mycelium is not affected by any medium thus far used, although growth rate is somewhat increased on the Westergaard and Mitchell medium, supplemented as noted above. Thus the factor *E* behaves as a pleiotropic gene, of a type more complex than previously encountered in *Neurospora*, and would seem perhaps to prove of interest in connection with certain genetic hypotheses now current.

Obviously the next step will be to find a still better medium on which two lethal *E* races of opposite mating type will grow and function in fertilization to produce asci which will be homozygous, *E/E*. Following reduction each nucleus will carry the dominant *E* factor for eight-sporedness, and each of the eight spores would contain a single *E* nucleus at its origin. This would mean that, so long as one inbreeds, an eight-spored race or type of *N. tetrasperma* could be propagated sexually indefinitely.

Independent analytic and cytogenetic research by one of the junior authors (Singleton) bearing on various aspects of questions brought up in this paper will be reported by him in forthcoming papers.

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INTERFERENCE PHENOMENA FOR PARTICLES OBEYING BOSE OR FERMI STATISTICS

MARCOS MOSHINSKY

Institutos de Física y Geofísica, Universidad de México

INTRODUCTION

It is well known from experiment, as well as from classical theory, that strictly monochromatic light waves originating from different sources do interfere.[1] An example in which the waves are nearly monochromatic is found in the case of radio waves, and it is known that radio waves from two stations that have the same frequency show an interference effect.

We may describe the phenomenon quantum mechanically if we represent the light quanta emitted by both sources by waves in Schroedinger configuration space. So long as we consider the quanta emitted by one source as different from those emitted by the other, the total wave function will be the product of the wave functions corresponding to the quanta emanating from both sources, and there will be no interference effect. In the present paper, we will see how things are changed when we symmetrize or antisymmetrize the wave function, i.e. assume either Bose or Fermi statistics for the particles.

In the simple quantum mechanical picture that we will use here, we will assume a definite number of photons coming out from each source, and this implies that the phase relations will be undetermined.[2] As a result, even the symmetrized wave functions must be expected to give, at any point in space, the same density of quanta that classical theory determines, when the intensity is averaged over the phase differences between the two sources. This implies that no interference is observed between the waves from the two sources.

If we want to look for an interference effect when dealing with a quantum picture in which we have a definite number of photons, we must look for a phenomenon whose classical description should be independent of the phase. We shall make use of the fact that, in classical theory, one can observe interference phenomena by observing the intensity at two points in space, even if the phases are distributed at random. The measurement of the light intensity at one point permits one to calculate the phase difference between the two sources, and this, in turn, gives the intensity at the other point.

What one must observe, according to classical theory, are the correlations of the intensities at different points. As we will see, these phase independent correlations take a particularly simple form if we consider the product of the intensities at two given points, and give rise to what we will call a second order interference effect. These interference effects can be described by the simple quantum theory employed here, and it is shown that one obtains by means of a symmetrized wave function the same correlations between the intensities which are postulated by classical theory. There is, of course, no second order interference for an unsymmetrized wave function, and although there is a second order interference effect for an antisymmetrized wave function, the pattern is different from that given by the classical theory. Hence, the validity of the classical interference theory is tied to the Bose behavior of the interfering particles.

CLASSICAL THEORY

Let us consider the correlations that appear in classical theory between the intensities at different points in space due to light waves originating from two distinct sources A and B.

The two sources A and B are separated by a distance R large compared with the wave length, r designates the distance of any point P from A, ρ its distance from B, and $\mathbf{r}$ is the position vector determining P with respect to some given fixed origin.

As we are only interested in the interference properties of radiation, we can assume that the field is described by a scalar function U satisfying a wave equation. If the total energies radiated per second by the sources A and B are taken

[1] Planck, M., Theory of light, 50, London, Macmillan, 1932.

[2] Dirac, P. A. M., The principles of quantum mechanics, 1st ed., chap. XII, Oxford, Clarendon Press, 1930.

proportional to n and m respectively, the classical field function U at the point P and time t has the form:

$$U(\mathbf{r}, t) = n^{\frac{1}{2}} r^{-1} \exp[2\pi i(r/\lambda - \nu t)] + m^{\frac{1}{2}} \rho^{-1} \exp[2\pi i(\rho/\lambda - \nu t) + i\varphi],$$

where λ is the wave length of the radiation, $\nu = c/\lambda$ the frequency and φ the phase difference between the monochromatic waves originating at A and B. The intensity at the point P becomes then:

$$I(\mathbf{r}, \varphi) = \frac{n}{r^2} + \frac{m}{\rho^2} + \frac{2n^{\frac{1}{2}}m^{\frac{1}{2}}}{r\rho} \cos\left[\frac{2\pi}{\lambda}(r - \rho) - \varphi\right]. \quad (1)$$

If we had strictly monochromatic sources, an interference effect would appear because of the last term in (1). However there are no strictly monochromatic light sources.

The breadth $\Delta\nu$ implies[3] that the radiation emitted by a source is in the form of wave trains of finite length L, of the order of magnitude $L \approx c/\Delta\nu$. As the radiation emitted from the sources A and B will consist of superpositions of finite wave trains, instead of the two infinite wave trains corresponding to the strictly monochromatic sources, the phase difference φ will suffer arbitrary fluctuations. As a result, the intensity actually observed is given by $I(\mathbf{r}, \varphi)$ averaged over the phase, which then becomes:

$$\bar{I}(\mathbf{r}) = n/r^2 + m/\rho^2. \quad (2)$$

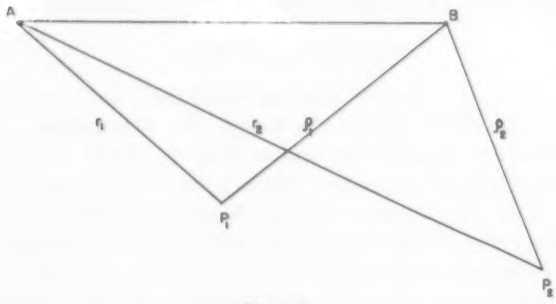

FIG. 1.

Let us consider now the correlations between the intensities at two given points, viz. at P_1 at time t_1, and at P_2 at time t_2. The light waves passing through P_1 at time t_1 originate at time $t_1 - r/c$ and $t_1 - \rho/c$ at A and B respectively. They give rise to an intensity $I(\mathbf{r}_1, \varphi_1)$ given by (1). The light waves passing through P_2 at time t_2 originate at times $t_2 - r_2/c$ and $t_2 - \rho_2/c$ from A and B, respectively, and give rise to the intensity $I(\mathbf{r}_2, \varphi_2)$. In general, the phase differences fluctuate independently, except if

$$|(t_1 - r_1/c) - (t_2 - r_2/c)| \ll L/c \quad (3a)$$

$$|(t_1 - \rho_1/c) - (t_2 - \rho_2/c)| \ll L/c. \quad (3b)$$

If the condition (3a) is satisfied, we may assert that the same wave train originating in A is passing through P_1 at t_1 and through P_2 at t_2. A similar statement holds if (3b) is satisfied for waves originating at B. We see therefore, that $\varphi_1 \approx \varphi_2$ if (3) holds.

If we now consider an experiment in which the product of the intensities at the points P_1 and P_2 is measured, there are two alternatives: (a) If the times at which the intensities are considered at P_1 and P_2 are such that the condition (3) holds, then we have $\varphi_1 = \varphi_2 = \varphi$ and owing to the fluctuations of this phase difference, the product of the intensities which is measured, becomes:

$$\begin{aligned}\bar{I}(\mathbf{r}_1, \mathbf{r}_2) &= (2\pi)^{-1}\int_0^{2\pi} I(\mathbf{r}_1, \varphi) I(\mathbf{r}_2, \varphi) d\varphi \\ &= (n/r_1^2 + m/\rho_1^2)(n/r_2^2 + m/\rho_2^2) \\ &\quad + 2nm(r_1 r_2 \rho_1 \rho_2)^{-1} \\ &\quad \times \cos[2\pi(r_1 - \rho_1 - r_2 + \rho_2)/\lambda].\end{aligned} \quad (4a)$$

(b) On the other hand, if (3) is not satisfied, φ_1 and φ_2 fluctuate independently and the product of the intensities becomes:

$$\begin{aligned}&\bar{I}(\mathbf{r}_1, \mathbf{r}_2) \\ &= (2\pi)^{-2}\int_0^{2\pi}\int_0^{2\pi} I(\mathbf{r}_1, \varphi_1) I(\mathbf{r}_2, \varphi_2) d\varphi_1 d\varphi_2 \\ &= \bar{I}(\mathbf{r}_1)\bar{I}(\mathbf{r}_2) \\ &= (n/r_1^2 + m/\rho_1^2)(n/r_2^2 + m/\rho_2^2).\end{aligned} \quad (4b)$$

From (4a) we see that there is a phase independent correlation between the intensities at the points P_1, P_2 whenever (3) is satisfied. We will refer to this phenomenon as the second order interference effect. We now proceed to a quantum mechanical picture for this second order interference effect.

QUANTUM THEORY

We now look at the picture of radiation coming from two sources from a quantum mechanical point of view. We assume that the sources A and B during their time of operation have

[3] Born, M., *Optik*, 134, Berlin, Springer, 1933.

emitted n and m particles respectively, with definite energy. These particles obey some type of statistics to be specified later. The system of $n + m$ particles is described in Schroedinger's configuration space of $3(n + m)$ dimensions.

The quantum theory of radiation interprets the classical intensity $\bar{I}(\mathbf{r}_1)$ as $h\nu dt^{-1}$ times the probability $P(\mathbf{r}_1)$ of observing a light quantum at P_1 during the time interval dt. According to this relation between intensity and probability, $\bar{I}(\mathbf{r}_1, \mathbf{r}_2)$ has the simple interpretation of $(h\nu)^2 dt_1^{-1} dt_2^{-1}$ times the probability $P(\mathbf{r}_1, \mathbf{r}_2)$ that we observe a light quantum at P_1 during the interval dt_1, as well as a light quantum at P_2 during dt_2.

$P(\mathbf{r}_1)$ and $P(\mathbf{r}_1, \mathbf{r}_2)$ can be computed directly from the Schroedinger wave function in configuration space. The square of the absolute value of this wave function $|\psi|^2$ represents the probability density of finding each one of the particles at the point specified by the variables in ψ. The probability density of observing only two particles at definite points will be obtained by averaging $|\psi|^2$ over all the coordinates in Schroedinger configuration space, except those corresponding to these two particles. Similarly $P(\mathbf{r}_1)$ is obtained by averaging over all the coordinates in Schroedinger configuration space, except one.

Thus $P(\mathbf{r}_1)$ and $P(\mathbf{r}_1, \mathbf{r}_2)$ are obtained by averaging over Schroedinger's configuration space, while $\bar{I}(\mathbf{r}_1)$ and $\bar{I}(\mathbf{r}_1, \mathbf{r}_2)$ were obtained by averaging over the phase. Our question then is whether $\bar{I}(\mathbf{r}_1)$ is identical with $P(\mathbf{r}_1)$, and $\bar{I}(\mathbf{r}_1, \mathbf{r}_2)$ with $P(\mathbf{r}_1, \mathbf{r}_2)$. It will be seen that the latter quantities agree only if Bose statistics is used for the particles. The quantities $\bar{I}(\mathbf{r}_1)$ and $P(\mathbf{r}_1)$ agree, on the other hand for all types of particles, no matter whether they obey Bose, Fermi, or Boltzmann statistics.

BOSE STATISTICS

In determining $P(\mathbf{r}_1)$ and $P(\mathbf{r}_1, \mathbf{r}_2)$ in the case of Bose statistics, we can assume a definite energy, i.e., a definite wave length λ for the n and m particles emitted from A and B respectively. The wave functions for each particle will satisfy the time independent Schroedinger equation:

$$\Delta\psi + k^2\psi = 0, \qquad k = 2\pi/\lambda. \tag{5}$$

The solution of (5) which represents an outwardly traveling infinite wave train originating at A, is given by $r^{-1} \exp ikr$, and the one that represents a wave train originating at B is $\rho^{-1} \exp ik\rho$.

In the case of Bose's statistics the wave function of the system will be symmetric, and is given by the expression

$$\begin{aligned} &C'^{\frac{1}{2}(n+m)}\psi(\mathbf{r}_1, \mathbf{r}_2, \cdots, \mathbf{r}_{n+m}) \\ &\quad = (n!m!(n+m)!)^{-\frac{1}{2}} \\ &\quad \times \textstyle\sum_p (r_1 \cdots r_n \rho_{n+1} \cdots \rho_{n+m})^{-1} \\ &\quad \times \exp ik(r_1 + \cdots r_n + \rho_{n+1} + \cdots + \rho_{n+m}), \end{aligned} \tag{6}$$

where $\sum_p$ represents a summation over all permutations of the $n + m$ indices, C' is a normalization constant.

We are concerned with the probability of finding two particles, one in the neighborhood of r_1, and the other in the neighborhood of r_2, irrespective of the positions of the other particles. This probability will be given by

$$\begin{aligned} P(\mathbf{r}_1, \mathbf{r}_2) = \int \cdots \int |\psi(\mathbf{r}_1, \mathbf{r}_2, \cdots, \mathbf{r}_{n+m})|^2 \\ \times d\mathbf{r}_3 d\mathbf{r}_4 \cdots d\mathbf{r}_{n+m}. \end{aligned} \tag{7}$$

In the process of evaluation of (7), we must consider the relative order of magnitude of the integrals of the type:

$$C' = \int d\mathbf{r}/r^2; \quad C'' = \int (r\rho)^{-1} \exp ik(r - \rho) d\mathbf{r}. \tag{8}$$

Both integrals (8) diverge if the volume of integration V is actually infinite. This corresponds to the presence of infinitely many particles in the system, and is the consequence of the assumption that the emission of particles has proceeded at a finite rate, for an infinitely long time.

If we assume that the emission started only a finite time ago, the volume of integration V will be finite but very large. Under this assumption, C'' is clearly negligibly small as compared with C', because of the oscillating exponential in its numerator.

To evaluate (7), let us consider the terms of $|\psi|^2$ individually. Of the $(n + m)!$ terms of ψ we will first consider those in which the indices 1 and 2 are affixed to r. There are $n(n - 1)(n + m - 2)!$ such terms. Now, because $C'' \ll C'$, the only terms of ψ^* which will contribute to the integral (7), when they are combined with the terms of ψ of the above type, are those in which the indices of r and ρ in ψ^* are permutations of the respective indices of r and ρ in ψ. For each term in ψ there will exist $n!m!$ terms in ψ with which it can combine, giving contributions to (11). There are, therefore, $n(n - 1)(n + m - 2)!n!m!$ terms in $\psi\psi^*$

in which the indices 1, 2 are affixed to r and which contribute to the integral (11). As each of these terms has the form:

$$[n!m!(n+m)!r_1^2r_2^2\cdots r_n^2\rho_{n+1}^2\cdots\rho_{n+m}^2]^{-1} \quad (9a)$$

their contribution to (11) becomes

$$\frac{n(n-1)C^{l-2}}{(n+m)(n+m-1)r_1^2r_2^2}. \quad (10a)$$

For similar reasons, if the indices 1 and 2 are affixed to ρ we get a contribution:

$$\frac{m(m-1)C^{l-2}}{(n+m)(n+m-1)\rho_1^2\rho_2^2}. \quad (10b)$$

Let us now consider the terms of ψ in which we affixed the index 1 to r and the index 2 to ρ. There are $nm(n+m-2)!$ terms of this nature. The number of terms in ψ^* that in combination with one of the above terms of ψ will contribute to (7), will be $2n!m!$. As a result, we have $nm(n+m-2)!n!m!$ terms of the form:

$$[n!m!(n+m)!r_1^2\rho_2^2r_3^2\cdots r_{n+1}^2 \times \rho_{n+2}^2\cdots\rho_{n+m}^2]^{-1} \quad (9b)$$

and the same number of terms of the form

$$\frac{[n!m!(n+m)!]^{-1}\exp ik(r_1-\rho_1+\rho_2-r_2)}{r_1\rho_1r_2\rho_2r_3^2\cdots r_{n+1}^2\rho_{n+2}^2\cdots\rho_{n+m}^2}. \quad (9c)$$

These, when integrated, contribute to $P(\mathbf{r}_1,\mathbf{r}_2)$ the term

$$\frac{mnC^{l-2}}{(n+m)(n+m-1)} \times\left(\frac{1}{r_1^2\rho_2^2}+\frac{\exp ik(r_1-\rho_1+\rho_2-r_2)}{r_1r_2\rho_1\rho_2}\right). \quad (10c)$$

The last term of our set is obtained by interchanging 1 and 2 in (10c).

Equs. (10a), (10b), (10c) show that, except for a normalization factor, $P(\mathbf{r}_1,\mathbf{r}_2)$ assumes the form

$$P(\mathbf{r}_1,\mathbf{r}_2)=\frac{n(n-1)}{r_1^2r_2^2}+\frac{m(m-1)}{\rho_1^2\rho_2^2}+\frac{mn}{r_1^2\rho_2^2} +\frac{mn}{r_2^2\rho_1^2}+\frac{2mn\cos k(r_1-\rho_1+\rho_2-r_2)}{r_1r_2\rho_1\rho_2}. \quad (10)$$

If the number of photons emitted by each source is large $P(\mathbf{r}_1,\mathbf{r}_2)$ is equal to $\bar{I}(\mathbf{r}_1,\mathbf{r}_2)$ as given by (4a).

From (7) we see that the probability density for the observation of one photon is given by

$$P(\mathbf{r}_1)=\int P(\mathbf{r}_1,\mathbf{r}_2)d\mathbf{r}_2. \quad (11)$$

This is equal to the $\bar{I}(\mathbf{r}_1)$ of (2).

The correlations obtained above are of the same form as the ones that appear for the product of the intensities in the classical case. The classical interference phenomenon is, therefore, intimately connected with the Bose behavior of the photons.

BOLTZMANN STATISTICS

If the particles emitted obey Boltzmann statistics, they are distinguishable. Let us identify the particles emitted from the source A by the indices 1, 2, $\cdots$ n, and those coming from B by the indices $n+1$, $\cdots$, $n+m$. The wave function ψ representing the system of the $n+m$ distinguishable particles, is given by:

$$\psi(\mathbf{r}_1,\mathbf{r}_2,\cdots,\mathbf{r}_{n+m}) =\frac{\exp ik(r_1+r_2+\cdots+r_n+\rho_{n+1}+\cdots+\rho_{n+m})}{r_1r_2\cdots r_n\rho_{n+1}\cdots\rho_{n+m}}. \quad (12)$$

From (12), disregarding the normalization factor, we obtain by a calculation similar to the above

$$P'(\mathbf{r}_1,\mathbf{r}_2)=r_1^{-2}r_2^{-2}.$$

However, we are not interested in the probability of finding particle 1 at r, and particle 2 at r_2, but in the probability $P(\mathbf{r}_1,\mathbf{r}_2)$ of finding any one of the $n+m$ particles at r_1 and any other of them at r_2. This is $\sum_{ij}P'(\mathbf{r}_i,\mathbf{r}_j)$. Hence

$$P(\mathbf{r}_1,\mathbf{r}_2)=\left(\frac{n}{r_1^2}+\frac{m}{\rho_1^2}\right)\left(\frac{n}{r_2^2}+\frac{m}{\rho_2^2}\right). \quad (13)$$

This differs from (11) by the last term thereof. It was this last term which gave the second order interference effect, this permitting the identification of $P(\mathbf{r}_1,\mathbf{r}_2)$ with $\bar{I}(\mathbf{r}_1,\mathbf{r}_2)$.

However, we still have $P(\mathbf{r}_1)=n/r_1^2+m/\rho_1^2$ so that $P(\mathbf{r}_1)$ remains equal to $\bar{I}(\mathbf{r}_1)$.

FERMI STATISTICS

In this case we can no longer use wave functions of definite energy but have to consider wave packets. This makes a somewhat more detailed analysis necessary.

The rates at which Fermi particles are emitted from A and B are, respectively, n and m per unit time. If T is the time during which the sources A and B have been in operation the total number of particles emitted becomes $N=nT$ and $M=mT$.

We want the wave packet $\sigma_i(\mathbf{r},t)$ which represents a particle appearing at the point source A at time τ_i, to have the following properties:

a) It shall satisfy the time dependent Schroedinger equation.

b) It shall represent a particle emitted at time τ_i.

c) It shall represent a particle with a sharply defined wave number k_0 as far as this is consistent with a coherence length L, as discussed in the introduction.

For a point P whose distance from A is much larger than L, the $\sigma_i(\mathbf{r}, t)$ may be represented by

$$\sigma_i(\mathbf{r}, t) = \frac{1}{(2\pi)^{\frac{1}{2}}} \int_0^\infty \times \frac{\exp[ikr - ihk^2(t - \tau_i)/2\mu]}{kr} g(k)k^2dk. \quad (15)$$

Where μ is the mass of the particle and $g(k)$, which is the spectral distribution in momentum space, has a narrow maximum at $k = k_0$ whose width Δk is of the order of magnitude of $1/L$; (5) satisfies condition (*b*) because of the exponential factor.

Similarly, a particle originating at B at time τ_j will be represented by

$$\sigma_j(\mathbf{r}, t) = \frac{1}{(2\pi)^{\frac{1}{2}}} \int_0^\infty \times \frac{\exp[ik\rho - ihk^2(t - \tau_j)/2\mu]}{k\rho} g(k)k^2dk. \quad (16)$$

It is clear that the $N + M$ wave packets $\sigma(\mathbf{r}, t)$ must be approximately orthogonal. First, the wave packets emitted from a single source cannot overlap because of the exclusion principle, and second, wave packets coming from two different sources overlap effectively only in a very small region of space, if the distance between the sources is large compared to $1/k_0$. By normalizing $g(k)$ appropriately we may assert that $\sigma_\kappa(\mathbf{r}, t)$ for $\kappa = 1, \cdots, N + M$ form an approximately orthonormal system.

Proceeding as in previous sections, we construct the antisymmetric function representing this system of $N + M$ particles, with the difference, however, that we now associate a definite time of observation with each point of observation, and we have:

$$\psi(r_1, t_1, \cdots, r_{N+M}, t_{N+M}) = (N + M)!^{-\frac{1}{2}} \text{Determinant}||\sigma_\kappa(r_j, t_j)||. \quad (17)$$
$$(\kappa, j = 1, 2, \cdots, N + M).$$

Thus $\psi\psi^* d\mathbf{r}_1 d\mathbf{r}_2 \cdots d\mathbf{r}_{N+M}$ represents now the probability of observing a particle in the neighborhood $d\mathbf{r}_1$ of $\mathbf{r}_1$ at time t_1, another in the neighborhood $d\mathbf{r}_2$ of $\mathbf{r}_2$ at time t_2, etc. The probability density $P(\mathbf{r}_1, t_1, \mathbf{r}_2, t_2)$ of observing two particles at these points at definite times is given by an integration over configuration space as in (7). Taking into account the orthonormal properties of $\sigma_\kappa(r_j, t_j)$ we are led to the result[4]

$$P(\mathbf{r}_1, t_1, \mathbf{r}_2, t_2) = [2(N + M)(N + M - 1)]^{-1} \times \sum_{\kappa, j=1}^{N+M} |\sigma_j(\mathbf{r}_1, t_1)\sigma_\kappa(\mathbf{r}_2, t_2) - \sigma_j(\mathbf{r}_2, t_2)\sigma_\kappa(\mathbf{r}_1, t_1)|^2. \quad (18)$$

The probability of observing one particle in the vicinity of $\mathbf{r}_1$ at time t_1 is given by (11*a*) and (18) as:

$$P(\mathbf{r}_1, t_1) = (N + M)^{-1} \sum_j |\sigma_j(\mathbf{r}_1, t_1)|^2. \quad (19)$$

For the evaluation of these probability distributions, the above summations are replaced by appropriate integration with respect to the time τ of emission of the particles. These integrals may be evaluated under the assumption that t_1, t_2 are well within the time interval O, T so there does not appear any effect on the probability distribution due to the finite time of operation of the sources. It is also assumed that the spreading of the wave packets is not appreciable, i.e.,[5] that $th\Delta k < \mu L$ or $t_1, t_2 < k_0L^2/v_0$ with $v_0 = hk_0/\mu$ the average velocity.

The probability distribution, omitting the normalization factor, then takes the form

$$P(\mathbf{r}_1 t_1, \mathbf{r}_2, t_2) = (n^2/r_1^2 + m^2/\rho_1^2)(n^2/r_2^2 + m^2/\rho_2^2) - \frac{1}{Q^2} \int_0^\infty \int |k'k''g(k')g(k'')|^2 \times \left\{ \frac{n^2 \cos A}{r_1^2 r_2^2} + \frac{m^2 \cos B}{\rho_1^2 \rho_2^2} + \frac{nm \cos C}{r_1 \rho_1 r_2 \rho_2} \right\} dk'dk'',$$

where

$$\begin{aligned} A &= (k' - k'')(v_0t_1 - r_1 - v_0t_2 + r_2) \\ B &= (k' - k'')(v_0t_1 - \rho_1 - v_0t_2 + \rho_2) \\ C &= \tfrac{1}{2}(k' + k'')(r_1 - \rho_1 - r_2 + \rho_2) - \tfrac{1}{2}A - \tfrac{1}{2}B \\ Q &= \textstyle\int_0^\infty |kg(k)|^2 dk. \end{aligned} \quad (20)$$

From (11*a*) and (19) we also obtain

$$P(\mathbf{r}_1, t_1) = n/r_1^2 + m/\rho_1^2. \quad (21)$$

From the properties of $g(k)$ mentioned above, the integrals in (20) give an important contribu-

[4] Fock, V., *Zeits. f. Phys.* **61**: 140, 1930.

[5] Pauli, W., Die Allgemeinen Prinzipien der Wellenmechanik, Article in *Handbuch der Physik*, 2nd ed., **24/I**: 101, Berlin, Springer, 1933.

tion only in the neighborhood Δk of k_0. In this neighborhood $(k' - k'') < 2\Delta k$.

Let us now consider two limiting assumptions. First, that

$$\begin{aligned} v_0 t_1 - r_1 - v_0 t_2 + r_2 &\ll L \approx \Delta k^{-1} \\ v_0 t_1 - \rho_1 - v_0 t_2 + r_2 &\ll L \approx \Delta k^{-1}. \end{aligned} \tag{20a}$$

For the range of the variables k', k'' which concern us, we get A, $B \approx 0$ and substituting in (24), we obtain:

$$P(\mathbf{r}_1, t_1, \mathbf{r}_2, t_2) = nm \left\{ \frac{1}{r_1^2 \rho_2^2} + \frac{1}{r_2^2 \rho_1^2} - \frac{2 \cos k_0 (r_1 - \rho_1 - r_2 + \rho_2)}{r_1 \rho_1 r_2 \rho_2} \right\}. \tag{23a}$$

If we now consider the second limiting assumption

$$\begin{aligned} v_0 t_1 - r_1 - v_0 t_2 + r_2 &\gg L \approx \Delta k^{-1} \\ v_0 t_1 - \rho_1 - v_0 t_2 + \rho_2 &\gg L \approx \Delta k^{-1} \end{aligned} \tag{22b}$$

then such functions as $\cos A$, $\cos B$ are rapidly oscillating functions of k', k'' if k', k'' are in the interval Δk around k_0. The integrals containing these functions are not appreciably different from zero, so we get

$$\begin{aligned} P(\mathbf{r}_1, t_1, \mathbf{r}_2, t_2) &= P(\mathbf{r}_1, t_1) P(\mathbf{r}_2, t_2) \\ &= \left(\frac{n}{r_1^2} + \frac{m}{\rho_1^2} \right) \left(\frac{n}{r_2^2} + \frac{m}{\rho_2^2} \right). \end{aligned} \tag{23b}$$

From (23a) we conclude that a correlation exists for the probability of observation of two Fermi particles at given points. The conditions (22a), for the appearance of this correlation, are entirely analogous to those given by (3) for the classical radiation theory. We can speak, therefore, of an interference effect for Fermi particles. This is different, however, from the interference of Bose particles or that given by classical theory.

The probability of observing a single particle has exactly the same form as in the case of Bose or Boltzmann statistics. If the condition (22a) is not satisfied, (23b) shows that there is no correlation between any two particles.

The condition (22a) for the interference effect between Fermi particles can also be formulated as follows: The times $t_1 - r_1/v_0$ and $t_2 - r_2/v_0$, at which the particle is to be emitted from A to reach the points r_1 and r_2 at times t_1 and t_2, be closer to each other than corresponds to the duration L/v_0 of a wave train. A similar condition must hold for B. If these conditions hold, a kind of exchange effect can be expected which manifests itself in the form of the second order interference.

CONCLUSION

The present analysis shows that the correlations that appear in classical theory between intensities at given points, due to radiation coming from different sources, can be reproduced in the quantum picture only when we deal with Bose particles. The classical interference phenomenon which gives rise to these correlations is therefore intimately connected with the Bose behavior of the light quanta.

There also appear correlations for the probability of observing two particles in the case of Fermi statistics. These correlations appear under the same circumstances as in the classical case, though they are not of the same form. This interference phenomenon of the Fermi particles can be interpreted as a kind of exchange effect.

I wish to thank Professor E. P. Wigner for suggesting this problem and his kind interest in the development of it.

JOHN BRADBURY'S EXPLORATIONS IN MISSOURI TERRITORY*

H. W. RICKETT

The New York Botanical Garden

JOHN BRADBURY'S claim to botanical fame rests chiefly upon certain entries in the "Supplementum" of Frederick Pursh's *Flora Americae septentrionalis* (London, 1814). These supplementary pages contain a number of new species from that part of western North America which was called Missouri Territory or Upper Louisiana. After the description of each species appears a note on the locality and the specimen seen, e.g.: "On the banks of the Missouri. *Bradbury. v.s. in Herb. Bradbury.*" Pursh, then, had access to plants collected by Bradbury in North America, and used them as the basis of new species. Unfortunately this was done without the consent or even the knowledge of the collector, who always believed that his collections had been "pirated" from him, and that he had thus been deprived of the scientific honor due to his arduous explorations. The exact circumstances surrounding the disposition of the specimens have remained something of a mystery until the present time. The discovery of the letters quoted below has helped to explain what happened; besides illuminating other particulars in the life and character of Bradbury.[1] Examination of the specimens themselves, now scattered in several herbaria, has helped to establish types and type localities of a number of species.

Bradbury was born 20 August 1768 near Stalybridge in Lancashire.[2] Though even as a boy he had to earn his bread in the cotton mills, he early showed an interest in natural history, in which he attained some reputation. In 1792 or thereabouts, when he was aged twenty-four, he visited Sir Joseph Banks in London and was made a Fellow of the Linnean Society. He contributed many of the Cheshire records to Turner and Dillwyn's *Botanist's guide*, published when he was thirty-seven in 1805.[3]

I. CORRESPONDENCE

The twenty-three letters which follow were written between 1808 and 1813, the period of the initiation and completion of Bradbury's first journey to North America. Most of the letters were written by Bradbury himself to William Roscoe (1753–1831), one of the founders and the first president of the Liverpool Botanic Garden. A few letters are by his wife, Elizabeth, to the same address; a few by his son, John Leigh Bradbury; and a few by other persons concerned in his affair.

We discover Bradbury at the age of forty, as an amateur naturalist living with his family in Manchester, visiting Liverpool and working for William Bullock, borrowing works on ornithology. He has an established reputation for knowledge of local plants and animals; he has been for sixteen years a Fellow of the Linnean Society; and he already has in mind his great project of a voyage to North America, to collect "subjects in Natural History" in Kentucky and Louisiana. It is clear that he has no private means, that he must make any such venture not only an avocation but a livelihood; his first task, then, is to secure patronage; and this he hopes he has found in the trustees of the Liverpool Botanic Garden. He is thinking also of taking his son with him and setting him up as a gardener, to receive and care for the valuable living specimens to be collected in the wilderness, and to forward them to his patrons in England.

* The work was carried out with the aid of a grant from the Research Funds of the American Philosophical Society.

[1] The existence of the letters was called to my attention by Mr. H. Stansfield, Keeper of Botany in the Public Museums of Liverpool, during correspondence about the Bradbury collections still preserved in that institution. It is a pleasure to acknowledge here Mr. Stansfield's help in this study. At his instance and through the kindness of Mr. J. F. Smith, City Librarian, photostatic copies of the letters were made for me.

[2] For an account of Bradbury's life see R. H. True, A sketch of the life of John Bradbury including his unpublished correspondence with Thomas Jefferson, *Proc. Amer. Philos. Soc.* **68**: 133–150, 1929. Harshberger (*The botanists of Philadelphia*, 153, Philadelphia, 1899) has Bradbury a "Scotch naturalist." Additional particulars may be found in Britten and Boulger's *Biographical index of British and Irish botanists* (London, 1893), and in the *Handbook and guide to the herbarium collections in the Public Museums, Liverpool* (1935, p. 42, 43).

[3] Turner, Dawson & Dillwyn, Lewis Weston, *The botanist's guide through England and Wales*, London, 1805.

1. JOHN BRADBURY TO WILLIAM ROSCOE [4]

Liverpool, Museum 9 Sep 1808

Sir

Having now finished that part of Mr. Bullocks work which I undertook I have returned Boisson's Ornithologie which I trust you will find in as good order as when you so obligingly spared them.

When I accompanied Mr. Bullock to your house to ask for them he slightly mentioned that I had in contemplation a plan of going to Kentucky & Louisiana for the purpose of collecting subjects in Natural History in which undertaking I was under the necessity of looking for Patronage. I remind you of this Sir in order to introduce the subject again to your consideration that if there is anything therein deserving of your notice as being in trust for the Botanic Garden you may enquire farther into it. My plan is to examine those countries to the most remote settlements perhaps beyond and transmit to New Orleans whatever I may find which I deem not described throughout the whole range of Nat. History—At New Orleans I purpose to establish my Son as a Gardener to receive & send to Europe from time to time whatever is valuable—to propagate the plants and and send them in quantities in a Mature State. Finally I wish to find Patrons in the different branches of Nat. History who may conjunctly contribute to the expences of the undertaking in some kind of Ratio to the benefits they may expect to receive—Mr. Leigh Philips has already offered on the part of Entomology and Mr. Bullock encourages me to expect that Lord Stanley and himself will undertake for Mammalia and Birds. If it is not inconsistent with the plan of the establishment of the Botanic Garden I submit it to your judgment if there is anything in this to render it in the least likely that the Botanic part might be taken in its accounts. How far I am qualified for the undertaking is certainly a question of the first importance on that point I shall only say that it was not through any influence that 16 years ago the Linnaean Society did me the honor to elect me a Fellow more immediately Mr. Sheppard & Mr. Bullock can speak to it. but if it is decidedly foreign to the plan of the establishment to employ any of the funds to such a purpose I beg Sir you will have the goodness to inform me.

If I fail in this project (to which I am urged as much by necessity as choice) I beg to ask Sir if there is within your knowledge any situation which I might fill that would enable me to provide in a humble way for a numerous family? In this I do not look up to any acquirements in the Science of Nat. History as a qualification, rather otherwise, but I possess some others that perhaps are. Mathematicians call me a Mathematician and I have more particularly applied to that branch which is subservient to Mechanics—I do not *press* this point upon you Sir knowing well that applications of this nature are a kind of tax upon the benevolent; and to which the world makes you contribute largely—I shall quit Liverpool in three Days if Sir you would condescend to let me have a reply within that time it will relieve me from some anxiety.

I am Sir your most
obedient Servant
John Bradbury

The "numerous family" consisted, as will be seen, of a wife and eight children.

Bradbury had evidently obtained some assurance from the Liverpool Botanic Garden. According to True,[5] the Lancashire men in charge of the institution were motivated partly by a desire to find new lands for the growing of cotton. Of this no hint appears in the letters; Bradbury certainly had other reasons for proposing the scheme, which was obviously original with him. He then went to Dublin, apparently in search of additional means. He returned to Liverpool in December having received an offer of assistance apparently limited and qualified by conditions that he thought it impossible to accept. He writes to John Shepherd (1764–1836), first Curator of the Botanic Garden.

2. JOHN BRADBURY TO JOHN SHEPPARD

Liverpool 30th. Decr. 1808

Dear Sir

I arrived about an hour ago after a much longer stay in Ireland than I either wished or intended, and proceed immediately to Manchester to my family. I think it right thus hastily to inform you as the present medium of cummunication with the Committee that altho' I found the scientific men both belonging to the College and to the Dublin Society extremely favorable to my undertaking, yet nothing has been proposed by way of anticipating funds but what you will see indicated by the inclosed.[6] This aid I much wish to decline, partly out of respect to myself but much more in regard to your establishment whom I think it would be treating with disrespect to suffer the Dublin Society to rank with as Patrons under such circumstances. And if this sum only was necessary to compleat the requisite funds I could raise it amongst the Botanic Societies within the precincts of my own county, cheifly supported by the Weavers from whom

[4] The letters are here transcribed with all their idiosyncracies of spelling, punctuation, and capitalization, with only such concessions to modern orthography as the insertion of a few periods, the substitution of "Mr." for "M" and the like. A few illegible words, which do not affect the sense of any passage, are indicated by gaps thus: . . . .

[5] *Proc. Amer. Philos. Soc.* **68**: 136, 1929.

[6] No enclosure was found with the letter.

I would rather accept it. The board of Trinity College would certainly have contributed something had the matter been *pressed* upon them by my friends and particularly by Dr. Stokes who is one of the Senior Fellows & possesses much influence, but as they conceived themselves in trust for the Dead to whom they could not explain motives I thought it would be indecorous to commit my friends by urging the business. The Board have resolved to purchase of every thing sent and you are appointed to affix the price.

I have seen my sons who have given me every assurance of assistance and altho' I may be prevented by Death from doing justice to the undertaking *they will not abandon it*. This I trust will be considered as a good collateral security. They are very Healthy and athletic young men and fond of the science.

I beg you make the Committee acquainted with as much of the contents of this as you may deem necessary and inform them that I am ready to attend them at as early a period as they may be pleased to appoint. In the mean time I shall wait on Mr. Leigh Philips and do every other thing in my power to expedite the necessary arrangements. I beg I may hear from you as soon as possible and believe me

Your constant friend
John Bradbury

The following two letters are from William Parr Greswell, who was tutor of Mr. Dobson Lownder. The first is to "Will Roscoe," apparently the son of the William Roscoe to whom Bradbury addressed himself. The letters are concerned partly with this business, partly with literary matters. They are of interest here only as they mention Bradbury, and particularly as they call attention to his extreme indigence. With a characteristic well-to-do suspicion of poverty as a voluntary condition to be explained by drink or the devil, Greswell has inquired into possible causes of this financial deficiency, and can find nothing discreditable to Bradbury—presumably he was neither lazy nor profligate, and no explanation other than "misfortune" could be offered for his lack of means. In the second letter, to William Roscoe, passing reference is made to Bradbury's appointment by the Botanic Garden, which may thus be dated as anterior to 15 March 1809.[7]

3. WILLIAM PARR GRESWELL TO WILL ROSCOE

Denton near Manchr. March 3d. 1809.

My dear Sir

By the candid manner in which Mr. Roscoe was pleased to express himself in his last letter I am led to flatter myself that he will not think I do the Lownders any injustice in averaging my quarter's charge on Dobson's account in lieu of Notice at the even sum of Ten Pounds. If you will do me the favour to transmit to me a receipt upon a stamp, drawn up in such a form as you think satisfactory I will sign & return it to you; after which the amount of my demand may be inclosed in a post letter at your leisure & forwarded to me.

I shd. not have written on this subject till the qr. day had I not been desirous of mentioning that I have lately been amusing myself by looking a little into the History of early Parisian Printers, and particularly that of the "Stephani." I have collected a few notices respecting them, and am very desirous of consulting H. Stephani "Apologie pour Herodote—the best edition of which is I think, in your Father's possession. If I could make interest to borrow this work it might be sent with an old folio volume of mine "Adami vita" and two Octavo volumes by Bossu—unless Mr. Roscoe is likely to have further occasion for consulting them. The parcel, by Coach, might be addressed to my Brother at the College Manchr.

Do you know a Manchester Man of the name of Bradbury? He appears to be remarkably well informed on subjects of Natural History and Botany. It is said he is going out in a few weeks to America, to make collections & researches connected with these subjects, under the patronage of a society of Gentlemen of which your Father is one. I was sorry to hear to day in Manchr. that his family labours under considerable pecuniary distress—I am the more inclined to credit this report from my having lately seen him. His appearance sufficiently betrayed the "res angusta domi." I think him a very ingenious, and a modest man—and am told he stands uncharged with any imprudent habits—and that his poverty is the result of misfortune only. I should rejoice if anything could be done, in a delicate way to releive in some small degree his domestic difficulties.

I lately took up Cumberland's new review in a Bookseller's shop—and find some new Critic, *not anonymous*, wishes to acquire fame, by a new attack upon Leo. I am not convinced by his chemical metaphor, the only one that I recollect noticing in this very tame composition, that the mixture of Biography & History must necessarily neutralize both. It is in my judgment a contemptible critic too contemptible to deserve notice. What think you however of his well timed argumentative digression upon the Catholic question. Is the writer one of our order—if so he has as good a claim upon the Duke of Portland, almost as Mr. Bageley himself.

Please to present my respectful compliments to your hospitable fire side—and believe me

My dear Sir
Yours very faithfully
William Parr Greswell
Denton near Manchestr.

[7] A passage in letter 6 suggests February 17 as the date.

A note is added explaining the details of his charges for "board & Instructn."

4. WILLIAM PARR GRESWELL TO WILLIAM ROSCOE

College Manchr. March 15, 1809

My dear Sir

I was last night favoured with the receipt of your letter and a Bank note value Ten Pounds inclosed, on Mr. Dobson Lownder's acct. I return a receipt—upon the stamp sent; and if an acknowledgmt. in any other form be deemed necessary, shall be happy to give it when I next have the pleasure of visiting Liverpool.

I recognize your wonted benevolence in the notice which you are pleased to take of my mention of Mr. Bradbury. My acquaintance with him is but slight. I have seen him once or twice at Denton and occasionally elsewhere—And could not help being struck with the extensive information which he seems to possess on subjects of Nat. History. Of the circumstances of his family I have no personal knowledge. From a friend in Manchr. I accidentally heard that he has a wife & 8 children—and that they have suffered great privations—He has not the least knowledge of my having mentioned such a circumstance to you. I should not wish him to know it. Perhaps his feelings would be wounded by such a discovery. Of a resource so considerable as that which you mention being within his power—if I had had the least Idea, I should have considered my communicatn. respecting his case as altogether premature.

I am glad to hear of the appointment which he has obtained—and hope, that at length, Fortune may begin to smile upon him—and that his talents and exertions will meet with the reward which they seem to merit.

My indignation—or rather my contempt was strongly excited by a hasty perusal of Mr. B's invective. I agree fully with you in lamenting the present degraded state of periodical criticism. The public contempt or neglect will certainly—in a short time put an extinguisher upon Mr. Cumberland's luminous jumble, of church & state politics—hypercriticism &c. In looking so far back into our literary annals—this mushroom Critic seems to have paid you an unwilling compliment. He could find no recent production worthy to be made the subject of his profound censorial examination. He therefore agains calls out Leo X.

The books are safely arrived at Manchester—I am much obliged to you for the loan of l' "Apologie" &c. It shall be taken care of—and safely returned. Bossu is of no importance to me. I do not at all want it and therefore beg you will give yourself no trouble about it.

I remain—my dear Sir
With great respect—
Your obliged & faithful Servt.
William Parr Greswell.

We return to Bradbury with a brief note announcing his readiness to sail, some obstacle about which we are uninformed having been removed. We may assume that he sailed as he planned, probably late in April 1809 (certainly after April 25; see footnote 8 below). We next hear from him in that occidental Mecca of traveling scientists, Monticello. He had landed at Charleston, and had gone from there to Baltimore and Washington, meanwhile starting his baggage westward to Wheeling. At Monticello he stayed over two weeks, meeting Jefferson, to whom he presented a letter of introduction from Roscoe.[8] A number of excursions into the neighboring country, often in the company of Jefferson's son-in-law, Colonel Randolph, aroused his enthusiasm for the American flora. He discussed with Jefferson his proposed exploration westward, and learned what parts had and had not been visited by botanists. He accordingly abandoned New Orleans and made plans instead for St. Louis, intending for the wilderness beyond; being recommended to Meriwether Lewis, then Governor of Louisiana.

5. JOHN BRADBURY TO ? WILLIAM ROSCOE

April 20 1809

Sir

The obstacle which has for some time prevented my proceeding to America is now compleatly removed and in such a way that it cannot in future be any impediment to the execution of my plan.

I purpose that we shall sail in five or six Days and hope we may in the mean time obtain the Letters which it was proposed we should take with us. If you Sir or any of the Gentlemen belonging to the Garden will assist us in obtaining a passage on reasonable terms it would serve us materially.

I am Sir your most
Obedient Servant
John Bradbury

6. JOHN BRADBURY TO WILLIAM ROSCOE

Monticello 12th Augt. 1809.

Sir

I wrote to Mr. Sheppard from Charles Town by the hands of Monsr. Lherminier[9] a French naturalist who I hope will have called before the arrival of this and have delivered also a few Seeds I left also with Monr. Noisette[10] of the Botanic Garden there an

[8] *Proc. Amer. Philos. Soc.* **68**: 136, 1929. The letter was dated 25 April 1809 and received in Monticello 6 August 1809.

[9] Felix Louis L'Herminier, 1779–1833.

[10] Philippe Noisette, Michaux' assistant in the Charleston garden.

Introductory Lr. to promote a correspondence between Mr. Phipps and himself believing that should it take place it would materially benefit the Institutions.

From Charles Town I proceeded to Baltimore by sea from whence I forwarded my trunks &c. &c. to *Wheeling* on the Ohio a distance of 320 Miles and proceeded myself thro' Maryland and the Federal City to this place (about 230) which is the seat of Mr. Jefferson—I have been here about 10 Days which time I have spent in examining the neighborhood assisted often by Colnl. Randolph who is Son in Law to Mr. Jefferson and one of the best if not *the best* Naturalist I have met with. I have made out two new Cypripedia, two new Orchidea of a Genus established by *Persoon* which he has separated from *Serapias* and calls Heleborine, two new *Cacalia* a new and most Odorous Coreopsis a new Talinum a *beautiful* Rock Plant and many others of which I am doubtful. Some of these are removed into Mr. Jefferson's Garden and others are marked in the Woods & known to Col R who has this morning promised to take care of them for me, or your use should I never return to reclaim them—I learn from Mr. Jefferson that Capn. Lewis who he sent out to explore the Missouri to its source and from thence down the River Columbia to the South Sea is now Governor of Louisiana and lives at Fort St. Louis near the mouth of the Missouri. I learn also that Louisiana has not as yet been explored by any Botanist. I perceive from the Habitats given by Michaux in his Flora Americana Boreali that Kentucky, the Tennassee & Illinois have all been in some degree explored by him—from these considerations I have determined to go direct from hence through the Wilderness to Louisville in Kentucky there await my Luggage cross the Ohio and meet the Mississippi about the Mouth of the Missouri at Fort St. Louis and put myself under the direction of Capn. Lewis to whom Mr. Jeffn. and Mr. Randolph will give me Letters [11]—As Fort St. Louis is nearly 1000 Miles from hence it is probable you Sir will not hear from me soon as I shall probably see no occasion to write until I arrive there which (calculating on some delay at Louisville) I judge will take 6 weeks. In the present aspect of affairs it is far from being unlikely that a war betwixt this Country and Great Britain may take place and my intercourse with the Garden altho' not interdicted may be much obstructed. Under these circumstances it may happen that the Committee may wish to avail themselves of their power to withold from me any farther support in which case I think it but just that they should give me timely notice that I may after the 17th of Febr. next either look for some other Patronage or Employ—As I am assured by Mr. Jefferson that even War with Britain should not obstruct my researches I propose to establish a Garden at Ft. St. Louis as a place of immediate deposit and place one of my sons at New Orleans to receive and transmit. If therefore the Committee resolve to discontinue it will be necessary that they appoint some one in New Orleans to whom I may send all the Plants collected before the 17th Feb. If they continue let them send me 50 Pounds to Dr. Dowe that it may arrive soon after Christmas as with having to take so very circuitous a Route & travelling being very expensive here my Funds are much reduced—I trust Sir I may be favored with a Lr. on this head addressed *Fort St. Louis, Louisiana.*

Mr. Jefferson will I suppose write to you he has some intention of sending you seeds of a New Variety of Zea Maize which was brought by Capn. Lewis from a vast distance beyond the mouth of the Missouri he found it with a Tribe of Cultivating Indians in Latitude 49° and a Country so much elevated as to render it almost a Greenland climate. Mr. J thinks it may be an immense acquisition to Britain and it will

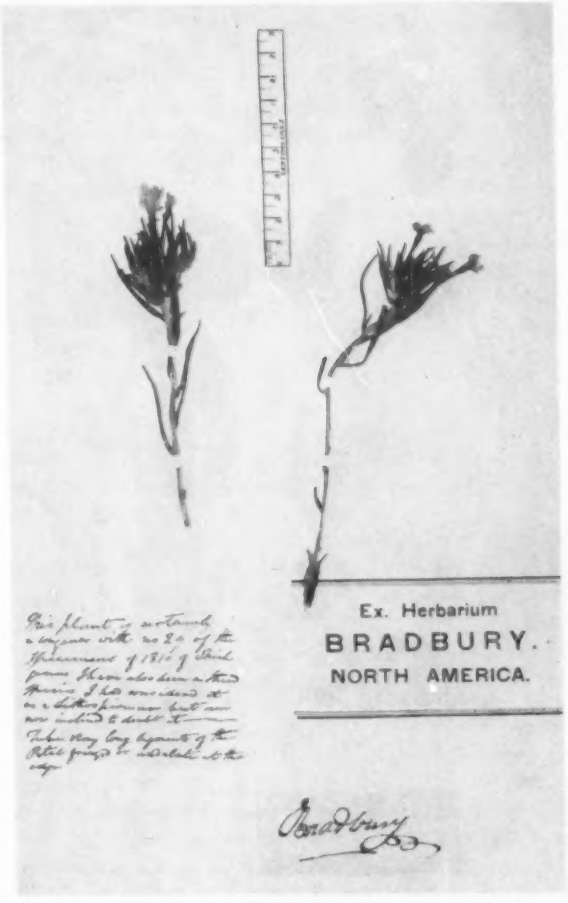

FIG. 1. *Lithospermum incisum* Lehm. [No. 95 (L)]. Isotype of *Batschia longiflora* Pursh. Annotated in Bradbury's hand.

[11] A letter of introduction from Jefferson to Lewis is quoted by True, *Proc. Amer. Philos. Soc.* **68**: 137, 1929.

grow even in the Highlands and yields most abundantly: At all events I shall not return to Europe without it—Coll. Randolph has obtained 2 years ago some seeds of *Holcus Sorghum* (*Sorghum* vulgare of Persoon) and of *Sesamum Indicum* both of which he is cultivating the former as an article of food the latter for Oil. The seeds of the Holcus are much better than the Maize & it yields in greater abundance I measured some of it feet high. The culture of both will become general and highly beneficial her[illegible].

I am Sir your very
obedient Servant
John Bradbury

P.S. Since I finished this Lr. Mr. Jefferson requested me to present his respectful compliments to you and informs me that he will forward all my Letters to Mr. Pinckney [12] which will afford me the opportunity of making inclosures I therefore send you Sir seeds of 5 Cypripedia Mr. Sheppard will observe that they are all inhabitants of shady woods.

FIG. 2. *Penstemon bradburii* Pursh [No. 106 (K)]. Isotype.

The interest in the introduction into England of new plants of economic value has the true Jeffersonian ring. Incidentally it is curious to notice that distinguished American conniving at the introduction of a presumably valuable food plant into a potential enemy country on the brink of war; a far cry from "total war." The new variety of "Zea Maize" mentioned above appears later as "Mandan Corn." From the high latitude [13] and the severe winter Bradbury was led to deduce something like a "Greenland climate" for what is now North Dakota, and supposed that the plant would therefore succeed in England. Of the history of this variety of maize in England I have no information.

The above letter is followed by a long silence, corresponding to (but exceeding) Bradbury's expectation—from August 1809 to May 1810. The gap is easily explained by the long journey from Monticello to St. Louis (his estimated six weeks ran to over four months); by bouts with the "ague"; and by adjustment to travel and life in the backwoods of America. Evidently he tested his endurance to the utmost, and evidently, in spite of much sickness, learned to take care of himself under primitive conditions; this is apparent also from his book, *Travels in the interior of America* (Liverpool, 1817). He had been known in England as an excellent marksman; and in America he prided himself on being able to support himself in the woods.

7. JOHN BRADBURY TO WILLIAM ROSCOE

St. Louis Louisiana May 10 1810

Sir

I remained only 6 days at Monticello after I wrote to you from thence, when I took my leave and travelled over that most Mountainous tract of country that lies betwixt Monticello and the falls of the Kenhawa (Kanawha) River a distance of about 240 Miles during my passage over these Mountains I found the Oil Nut (*Juglans cinerea*) mentioned by *Michaux. Jun.* and also discovered a new species of Corylus. I passed down the Kenhawa to its junction with the Ohio at point Pleasant 90 Miles from the falls here

[12] See Jefferson's letter to Pinckney cited by True, *Proc. Amer. Philos. Soc.* **68**: 141, 1929.

[13] Fort Mandan, where Lewis and Clark wintered, was nearer lat. 48°; but of course the corn may have been brought in from farther north.

I bought a skiff which is a small Boat worked by one person in which I rowed myself to the falls of the Ohio (Louisville) 325 Miles where I arrived the 27 Sepr. In a few days aterwards I fell sick of an Ague which was very severe and remained with me untill the 20 of November. On the 23 I quitted Louisville in a Boat down the Ohio in which I sailed to Shawaney Town (Shawneetown) about 10 Miles below the mouth of the *Wabash* and 350 from Louisville. here I quitted the River & got a Horse on which I travelled through an uninhabited country to Kaskaskia 145 Miles sleeping on the ground. At Kaskaskia I crossed the Mississippi into upper Louisiana and arrived here Decr. 31. From the beginning of Decr. to the end of March the Weather is more severe here than usually in that season in England the Mississippi here being often frozen over so that the Illinois farmers cross it with loaded Waggons on the Ice altho it is more than a mile in breadth and extremely deep and rapid. I therefore employed myself during the months of Jany. Feby. & March in collecting Birds & in preparing my Garden when the Weather permitted—I think I have been tolerably successful in Birds having near 60 species 8 or 9 of which I consider non discripts—About a Month ago the first flowers began to appear and now the Priaries [14] are truly beautiful I beg leave to observe that from the Atlantic to a line passing up the Mississippi, 300 Miles up the Ohio and then northward this Country (excepting what is cleared by the hands of man) is one immense forest of Trees: from the line above mentioned to the Pacific Ocean it abounds in Natural Meadows (Priaries) of from one Hundred to many Hundred Thousand Acres in extent. On one of these at a Plantation 3 Miles from St. Louis I have fixed my Garden which is now filling very fast with rare & non discript Plants of the latter discription I already count upon 16 species viz. of *Ribes*, Viola, Phlox, Cardamine, & Cypripedium 2 species in each: a Lithospermum, a fine Podalyria an Erigeron and an Hieracium I have also what I esteem most two Plants each of which will form a new Family in Didynamia Angiosperma. I speak here only of such plants as are actually in my Garden and which I have clearly ascertained as far as I am carried by the last Edition of the Systema Naturae and my own knowledge. Having entered into a correspondence with Dr. Dowe of Orleans I purpose to send to him to be forwarded to you Sir specimens from time to time during the ensuing summer which I trust I may have opportunities to do frequently as Boats are often passing from hence to Orleans and altho the distance is 1300 miles the passage is made in 20 Days probably you may receive the first parcel before you receive this Letter. I yesterday Planted the *Mandan Corn* of which I wrote to you by desire of Mr. Jefferson I have no doubt of its doing very well in England & it will be an immense National benefit they raise of it two Crops here in the same year the place from which it was brought is 1600 Miles up the Missouri and from the discription given me by the French Traders & the Hunters an excessively cold country: They reckon here and up the Missouri Six Species of Grapes some excellent, one I discovered about 3 weeks ago about 60 Miles up the Missouri the fruit of which the Back Settlers inform me will be ripe this month I purpose to send you roots and cuttings this Autumn I have no doubt but it will ripen its fruit in England—the Summer Grape of this Country if it answers the discription is very fine I cannot yet ascertain which speicies it is or whether discribed at all the leaves are some simply Heart-shaped, others 3 & again others 5 lobed—I hear also of a Plum which grows in the Priaries about 700 Miles up the Missouri which is about the size of an Hen's Egg and excellent the Shrub that bears it about 4 feet high & not thicker than a man's finger. If I can get it no other way I will go for it. The species of Lithospermum [*L. canescens*] mentioned above is a very handsome plant with Orange coloured flowers the Root yields a most beautiful red colour & is doubtless that with which the Indians paint themselves and stain the Hair Feathers etc. etc. which decorate their quivers, Belts, Calumet etc.—I shall send some of the Root with the first specimens—The Roots of both the species of Cypripedium of this Country are pleasant & powerful bitters!!! We have here three species of Ash *F. americana* platycarpa [*caroliniana*] & quadrangularis [*quadrangulata*] of Juglans & Quercus I have not yet determined but of each we have one species which I judge is not discribed. Of other Trees we have 3 species which at present I cannot tell where to arrange not having the fructification I expect to send to the Garden young plants of them having sowed them & two of the species are up—The common names are the Coffee nut [*Gymnocladus dioica*] and two kinds of Hack berry.

I wait for the determination of the Committee with a considerable degree of anxiety but whatever it may be I rest satisfied with myself conscious of having discharged my duty to the utmost of my power so far even to the endangering my health having provoked another attack of the Ague which however I have overcome after about 12 Days Sickness. If the Committee determine to support the plan I think they ought to appoint some House in Orleans to receive & ship the plants as I purpose to send from 100 to 150 or 200 each of the more beautiful non discript plants and have now actually in my Garden betwixt 6 and 700 of the two Species of Phlox and the Cypripidiums.

I mistook when I said above I had overcome the Ague, I have been very sick since but trust it is now leaving me. I was yesterday honored with yours of the 9th of December for which I cannot sufficiently

[14] It will be noticed that Bradbury is quite consistent in this mis-spelling.

thank you as it conveys to me the approbation of the Committee the recollection of it will often cheer me under hardships & difficulties that none but an *Enthusiast* would or perhaps could undergo—You mention Sir the Variety of Zea Mays known here by the name of Mandan Corn. I think Mr. Jefferson expected it from Governor Lewis it will not therefore be in his power to send it. But you may expect Sir from me some of it say a Bushel or two with the first parcel of Plants. Dr. Dowe also inclosed me a Dft. on Philadelphia for 222 22 Cents which I much wanted.

I am Sir with the greatest
gratitude and respect
your obedient Servant
John Bradbury

P.S. You will now Sir hear from me frequently.

In spite of the statement made in the above postscript, more than two months slip by before the next letter. The reason is not hard to find. Bradbury continued to be troubled by the "ague" (as is even more apparent from his wife's letters). Not having any new plants to communicate, he sends as a sop to his conscience a specimen of buffalo wool, in the hope that some manufacturer of cloth in England can find a use and a market for it.

8. JOHN BRADBURY TO WILLIAM ROSCOE

St. Louis 24th. July 1810

Sir.

Since I had this honor in May I have been so much confined to this neighbourhood by Ill health as not to make any excursions worth communicating to you but having determined to lose no opportunity of being useful to the world in any way however remote from the immediate object of my mission I now write to you Sir principally on the subject of the inclosed. You will perhaps recollect that in my last I mentioned that this country consisted of Wood Land and Priarie; as we proceed westward the proportion of Priarie increases so much that from the Mandan Nation about 1600 Miles up the Missouri to the Rocky Mountains 1400 Miles farther West is all priaries excepting the alluvion of the Rivers. In Missouri the Buffaloe are in such prodigious numbers that various travellers have assured me that they have seen what they have computed to be from 20 to 50,000 in one view. that country is excessive cold in winter and the Buffaloe are not cloathed with hair but a kind of wool, a specimen of which I enclose as shorn off the animal, being confident that it might be brought into use in Manufactures. at present no use is made of it and if I was empowered to buy I could procure Hundreds of Packs in a Year. If you Sir are acquainted with any Clothier in Yorkshire or the West of England they would soon give their opinion.

General Clarke arrived here about three weeks ago from the Federal City since when I have had frequent opportunities of conversing with him and find him more intelligent in Natural History than from his few opportunities of intercourse might be expected. he assures me that up the Missouri the Country affords a most ample field for researches in Natural history the productions continuing to vary as we proceed westward. He showed me a few specimens of Trees collected on the Rocky Mountains at the head of the Missouri & on the River Columbia. Amongst which I was surprised to see the Larch (*Pinus* Larix) and the Norfolk Island Pine (*Cupressus Columnaris*) the latter of which he assures me attains the Heighth of 340 feet & the Trunk to be 12 feet in Diameter there are Immense Forests of it. He offers me every assistance in his power to facilitate my researches which is to me of immense advantage as his power is extensive being Commander in Chief of this Territory a space of not less than 700,000 Square Miles. Another advantage to me is the vast rapidity with which the population of the Whites is pushing Westwards; there has been for some years numerous Plantations on both sides of the Missouri to 90 Miles from its confluence with the Mississippi. This spring 18 Families proceeded up to Boon's lick 100 miles farther others I hear are preparing to settle still farther up. The Indians are very numerous up the Missouri, but Whiskey, the Small Pox and (probably more than either) the wars amongst themselves occasioned by being now more circumscribed in their Hunting Grounds are depopulating them very fast even now we expect soon to hear of the extermination of a powerful Tribe the Osages who are indeed the Ishmaelites of this Country they are of Gigantic stature mostly from 6 feet to 6 feet 8 inches and muster 1200 brave men or Warriors. Six Tribes are going to carry on a war of extermination against them, viz Shawanese, Delawares, Sioux, Missouris, Kickapoos & Pawnees. It is no uncommon thing in the annals of the Western Country that a Tribe shall be extirpated in a Day as they sometimes carry on war after the Mosaic plan.

I suppose Sir that Mr. Sheppard will inform you as far as he knows of the progress I am making in the fulfillment of my mission. I do not think that my sickness has materially impeded it. For being the more confirmed in my intention of fixing permanently near this place as I become better acquainted with its very advantageous position. It is my best plan to inform myself in the first place of the Plants of this neighbourhood. The most interesting parts of the Country I have not yet entered upon these are the Flint Hills which separate the branches of the Secondary & tertiary Rivers & are here called knobs where they stand single or dividing ridges where they form a chain. these are a different soil from the Priaries and afford a distinct habitat I shall pay them one visit in about a fortnight as I go for a New

FIG. 3. *Rumex venosus* Pursh [No. 29 (L)]. Isotype. The slip at the left is Shepherd's label. Diagnosis in lower left corner in Bradbury's hand.

and Curious Grape that grows on one of the branches of the Merrimac about 110 Miles from hence—Excepting the Mandan Corn no plant here interests me so much as the summer grape which will I am of opinion come to great perfection in England. I will send to Mr. Sheppard plenty of the roots & cuttings to some of which I hope Sir you will give place at Allerton I do not think there is in the World a Country rising faster than this nor one which posses more natural advantages and yet the price of the best land (if not in the vicinity of St. Louis) only 2 Dollars pr. Acre—the price of Wheat 1/2 a Dollar pr. Bushel Pork & Beef 2 Dollars pr. Hundred. The culture of Hemp & Tobacco is only commencing.

I am Sir most respectfully
Your obedient servant
John Bradbury

P.S. Since I began this the news is arrived that the Indians have killed a number of the Back Settlers about 30 miles from hence & I find from Genl. Clarke that we shall have war with 7 or 8 Tribes the Winnebagoes or Puauts are the greatest aggressors.

Meanwhile Elizabeth Bradbury, at home in Cork, was pining for her absent husband and doubtless having some difficulty in providing for his numerous offspring. In September she wrote to Roscoe, telling what she knew of her husband's travels and plans (which he had already, as we have seen, communicated to Roscoe himself); and expressing herself very touchingly and confidently as ready and willing to go out and join him (with all her brood), to help him in his life work and to care for him in health and sickness. The only thing requisite was an advance of his salary to pay for their transportation. Accordingly she enclosed an order on Lord Stanley for £100, sent by John Bradbury, of which £70 was to come to her, the remainder to be sent out to New Orleans. Even the £70 was quite inadequate to finance a trip to St. Louis for nine persons, and she was hoping for an advance of the next year's salary. A second letter in October was apparently necessary to follow up this request for money, and receipt of the £70 was finally acknowledged in December; all hopes of a more ample subsidy and of joining her husband having evidently faded in the meantime.

If the subservient phrases of Elizabeth's letters seem strange to modern ears, we must recall that it was the custom of the time so to mark the distinction between the classes. At least we can admire the eloquence of these compositions of an unlettered woman, which compare favorably with letters in modern "business English."

9. ELIZABETH BRADBURY TO WILLIAM ROSCOE

Mr. Roscoe.

Sir. I recei'd a Letter yesterday from St. Lowis dated May 17: 1810. from my Husband John Bradbury with an order on Lord Stanley for one hundred pounds which is enclosed in this by my Husbands desire, who stated that if I knew of no other mode of procuring payment, that, if I applied to you, you would have the kindness to procure it for me. I am sensible that it is too great a trespass on your time and goodness for which I can return you but barren thanks; if you would not like to interfere in it I shall feel equally grateful for I am satisfied that your refusal will proceed from motives as honorable as all your conduct is, and by returning me the Order I shall be obliged to directly apply for it to Lord Stanley himself which would be perhaps the most eligible mode to adopt now, as it would save you the trouble of an application, but in sending it to you I merely

comply with my Husband's desire. As he informs me that he receiv'd a Letter from you I suppose that he has written to you in answer, it will be therefore almost unnecessary to communicate any part of this Letter which is principally filled with a brief account of his Journeys. He has commenced his Garden within three miles of St. Lowis and has already laid out above half an Acre, and sowed about 150 species of American trees, shrubs, and plants; he feels a confidence of being able to introduce a number of plants of "real utility" many are in his possession, and others he has heard of from Hunters who have ascended the Missouri and its tributary Streams 1200, 1600 and 2000 Miles—he has been also successful for Lord Stanley in Birds having collected 60 or 70 species seven of which are new, two of these Ducks very beautifull he has likewise a new Quadruped and a new genus of insects &c. I hear with regret that he was atacked with an Ague and fever last September, which continued six weeks, he says that it was brought on by excessive fatigue and having drank too much cold water—and in May last he had another fit of sickness occasioned he thinks by over exertion in his new occupation—he wishes much that his Family was with him, which wish is truly reciprocal, as they would tend considerably to ameliorate his Condition, the Lads could assist him in his excursions, and in the Garden, and Superintend and manage it in his absence, while my Daughters and myself could add to his Domestic comforts and administer to his wants if afflicted by Illness; it terrifies me to think of his Situation, liable to Sickness, so far from his relatives whose attention is thus become doubly necessary for I know his anxiety to Serve his family is so great that he will not relax his labours till he may seriously endanger his health. Besides his Sons being under his care may be taught his profession and be enabled to provide suitably for themselves, he says he means to adopt the pursuit he now follows as a Business for life—if you receive the Hundred pounds my Husband would be obliged to you, if you would send him thirty pounds of it, and to send me the remaining Seventy he tells me to direct some cloathes to him at Dr. Dowes Orleans, so I suppose the same address will do for the Money, but you are perhaps acquainted already with his address—when I receive the Seventy pounds I could be ready to set off with my Family to America, only that Sum is totally inadequate for that purpose for after purchasing Cloathes for my Husband and family consisting of eight Children and acting in the most economical manner I could not calculate on having more than thirty pounds left which would be insufficient to pay for our passage independent of provisions &c. I must therefore give up all hopes of going unless the Committee of the Botanic Garden would kindly advance the year or even half years Salary that will be due in advance the 1st. January 1811—How to ask them for it I dont know for my Husband does not desire me, nor even hint at it, tho' I am certain that he would be highly pleased and truly grateful for it, and would of course give the suitable receipts for it, when apprised of it, and if my acknowledgement would be thought of sufficient validity I would instantly give it—If I wait until January next to go, there will be a loss of several months which I would particularly regret as my Husband intends visiting England at the end [?] of his three years which will be on January 1, 1812 he would therefore have but a short time to instruct his Sons in managing the Garden while he is in Europe—I fear Sir I have tired your patience by my reflections which altho' they concern me considerably they do not, nor ought not, to affect you even by recital.

I am Sir your obedient Humble Servant
E. Bradbury
Post Office Cork

Cork Sep 10
1810

The following was presumably enclosed with the above:

St. Louis Louisiana 12 May 1810

My Lord

Please pay to the order of Mrs. Elizabeth Bradbury one Hundred Pounds Sterling on account of

John Bradbury

To the Right Honble.
Lord Stanley

10. ELIZABETH BRADBURY TO WILLIAM ROSCOE

Mr. Roscoe
Sir.

I receiv'd a Letter from my Husband Sept 9th. enclosing an Order on Lord Stanley for one Hundred pounds, which he desired me to transmit to you, saying that you would have the goodness to receive it for me. I wrote to you Sept. 10 and enclosed the Order —As such a considerable time has elapsed without receiving your answer, I am apprehensive that the Letter has miscarried, or as the direction for me was to the post office here, (where almost all my letters are directed) I fear some other person has received it, either in my name or by mistake—with these apprehensions I trust you will pardon me for again troubling you for an answer as soon as you conveniently can—Believe me Sir I am truly ashamed to take up so much of your time, but if you are disposed to censure me, recollect that the cause may be traced to yourself for if *you* were not so Benevolent *I* should not be so obtrusive.

I am Sir with gratitude and respect your
Obliged Humble Servant
E. Bradbury

Cork
Oct 11
1810 at Mr. Samuels's Moores Street Hammonds Marsh

11. ELIZABETH BRADBURY TO WILLIAM ROSCOE

Mr. Roscoe
Sir.

Your letter of the 14th Inst. I receiv'd yesterday, and for the agreable news you communicate and the trouble yourself have taken I return you my most grateful acknowledgments—I would be obliged if you would Send me the Sum originally mentioned £70 for I received two Letters from my Husband since I wrote to you and he desired me to Send him £50, therefore £30 added to your generous gift will make up that Sum—I think the most eligible mode of sending it to me would be by your Bank Bill payable in London, which I could easily get Cash'd; or by an Order on a Bank here for the Amount. My Husband's last Letter was dated the 10th of August he was then in good health, he is much pleased in his choice of situation and his success in plants continues.

I am Sir, with respect and
if possible, with additional gratitude
your Obliged Humble Servant
E. Bradbury

Cork
Nov 20 1810
Direct to me at the Post Office, I have found it always safe.

12. ELIZABETH BRADBURY TO WILLIAM ROSCOE

Mr. Roscoe
Sir.

Your letter of the 24th Ult. I receiv'd on the 30th; inclosing your Draft for £70, which I very easily got cashed—your promptness in Sending it, together with the trouble you take in forwarding the £30 to my Husband demands the sincere thanks of your most Obedient

and Obliged Humble Ser't
E. Bradbury

Cork
Dec 4
1810

In the next letter Bradbury proposes to explore the Arkansas River, and hopes for £100 to make this possible. This must be in addition to the previous hundred mentioned in Elizabeth's last letter, all but £30 of which went to his family. The "Committee" were probably just realizing that an exploring venture can be expensive—perhaps even experiencing some surprise that Bradbury's activities were apparently envisioned as a business for life. Certainly they must have been disappointed that a steady stream of new living plants from Missouri did not at once materialize. The letter from John Leigh Bradbury shows that the question of funds was ever pressing, particularly for his own projected part in the enterprise, and that prospective patrons were showing a natural reluctance to invest in a venture which after nearly two years had yielded nothing but a bit of buffalo wool and a few grains of maize. Then follows the first letter from Bradbury which announces a shipment of plants. This was the seven packages mentioned on the first page of his *Travels*, resulting from his excursions within a radius of 100 miles from St. Louis during 1810. In this letter it is plain also that Bradbury's hopes of additional money were not realized, and that he had to abandon his projected journey to the Arkansas. In its place he had the good fortune to become associated with Hunt's expedition up the Missouri, which is described in detail by Irving in his *Astoria* (Philadelphia, 1836).

FIG. 4. *Elaeagnus commutata* Bernh. [No. 82 (L)] and *Shepherdia argentea* (Pursh) Nutt. [No. 83 (L)]. Bradbury's diagnosis applies to the latter species.

13. JOHN BRADBURY TO WILLIAM ROSCOE

St. Louis 28 Novr. 1810

Sir.

I send you herewith some varieties of Maize amongst which are two kinds which will I think succeed in England as they will frequently come to maturity here in 10 or 11 weeks whereas the common variety requires the whole summer I think it would perhaps be advisable to plant it in two parts one about the beginning of May the other about the middle.

I shall spend next summer on the Arkansas River which is wholly unexplored by any one whose object was Natural History I trust I may be able to send you a much more valuable collection on my return down the River and perhaps I may accompany it. If Sir either you or the Committee have any particular object to which you would wish me to attend I shall with pleasure obey your instructions.

In the mean time I am under the necessity of mentioning that it will considerably frustrate my plans if I do not receive a farther supply of Money before May at which time I purpose to set out from Ozark a Town on the Mouth of the Arkansas and shall have no intercourse with White People (unless accidental) until October—I shall have occasion for 100 Pounds as my Boat, my outfit, and the pay of an attendant will be something expensive I think Messr. Kenner & Henderson will be more proper than Dr. Dowe to hand the money to as more consistent with their business.

I am Sir your most
obed Servant
John Bradbury

14. JOHN LEIGH BRADBURY TO WILLIAM ROSCOE

Gloucester Feb 18 1811

Wm. Roscoe Esqr.
Sir.

As you kindly condescended to assure me, that writing to you would not be considered an intrusion, I take the liberty of doing so.

On my way to London I called on a friend here, one of the first *Pin makers* in England who pressed me so strongly to spend some time with him, that I sent my letters franked to London, knowing that my presence was not absolutely necessary to forward my father's business. I have since heard from London that application has been made to the Linnean Society for their Patronage, the great, and I believe the only obstacle to which, is the unfortunate circumstance of nothing having been yet received from him. Can any thing be done Sir to satisfy them? Or has any thing yet arrived in Liverpool?

In regard to my-self nothing has yet appeared; the few friends I have are doing all they can; and in the mean time I am striveing to effect a revolution in a business that has not received any improvement, I supose for 2 Hundred years, and which wants it exceedingly.

You offered me when in Liverpool some of the Wool my Father sent over: I am sorry I did not accept it, and if you would not think it too much trouble I shall be much obliged to you if you would inclose me a little.

Permit me here to acknowledge the favours our family has received at your hands; received more, that your benevolence falls on all that come within its influence, than from any claim it could have on your goodness; favours for which I shall never have abilities to thank you as I would wish or oportunity I am afraid to deserve.

I am Sir your Obt.
Servant
J. L. Bradbury

At Mr. Charles Weavers
Gloucester

15. JOHN BRADBURY TO WILLIAM ROSCOE

St. Louis 25th Feby. 1811.

Sir.

Before you receive this I trust the collection which I made last summer will have arrived safely altho several untoward circumstances have prevented its progress in the early part so speedy as I could have wished—About a month ago news arrived here that the Boat which conveys it to Orleans had through the inexperience of the Patteroon grounded by taking the wrong side of an Island near the Mouth of Kaskaskias River about 80 Miles down the River to which place I rode but found that in about 3 days after the accident happened she got off & proceeded safe on her Voyage. I suffered much during my Journey from the extreme severity of the Weather the Thermometer having been mostly during the time below 0 and for one day 6° below in the Room in which I sleep.

This is probably the last time I shall have this honor untill October as I shall set out in a few Days along with a Hunting party who ascend the Missouri in their route to the Pacific, my present intentions are not to proceed farther than about 1600 Miles at which distance the Volcanic country occurs where I purpose to spend the summer and shall live with the *Sioux* or *Mandan* Indians from whom if no other opportunity occurs I can procure a Canoe or Periogue to descend the River with my collection in Autumn. I did not in the first instance intend to have spent the Summer on the Missouri but to have travelled from its Waters to those of the Arkansas and finally to have descended the latter River but this plan my want of means for the present prevents me from executing which I the more lament as from the report of Hunters the country on the Arkansas and from thence to *Mexico* is extremely interesting: But the expenses

April 20 1809

Sir The obstacle which has for some time prevented my proceeding to America is now compleatly removed and in such a way that it cannot in future be any impediment to the execution of my plans.

I purpose that we shall sail in five or six Days and hope we may in the mean time obtain the Letters which it was proposed we should take with us. If you Sir or any of the Gentlemen belonging to the Garden would assist us in obtaining a passage on reasonable terms it would serve us materially.

I am Sir your most
Obedient Servant
John Bradbury

FIG. 5. Letter No. 5, John Bradbury to William Roscoe.

attendant on the dispatching my collection have so much reduced my Stock that I shall not be able to purchase the necessaries for this Journey, not corn Bread unless I obtain assistance.

Should the plants be delayed at Orleans it is not unlikely that some of the earliest may have been destroyed by germinating in the packages in which case I beg Sir you will speak to the Committee on the necessity of giving me early information of which Plants do not succeed, that on my return in autumn I may again collect them I have so arranged that any Letters or even remittances that may arrive for me here shall be taken care of.

Not being certain that I shall return to Europe next autumn I shall be happy if you inform me whether the Missouri Corn succeeds and you may rest assured Sir that I shall attend to the introduction of any object which may promise to be useful to Society.

I am Sir your very
humble Servt.
John Bradbury

Mr. Bridger a Gentleman of my acquaintance from Manchester is come to settle in this neighbourhood intending to establish a Sheep and Cheese Farm & has purchased on the Priaries at his request I write to beg you will let Mr. Sheppard have a small cask of hay seeds from Allerton Meadows.....Swedish Turnip & Seed wheat which I shall write to desire Mr. S will put on board some Ship for Orleans to the care of Kenner & Henderson for Saml. [?] Bridger of St. Louis—K & H do shipping business for Mr. Bridger.

16. JOHN BRADBURY TO WILLIAM ROSCOE

St. Louis 10 March 1811

Sir

I write to you at the request of my Friend Mr. Charless the Printer of the Louisiana Gazette who has published therein some sketches of this Country, of which the World in general are so ignorant that in my opinion whatever will tend to throw a light upon its History must be desirable. In conformity with this feeling I make known to you his wish to republish the Sketches in England and to put the book under your Patronage so far as the causing it to be shewn to such of the Booksellers as might be likely to purchase the copy Right for the value of which Mr. Charless would willingly be reimbursed in Types I forbear Sir to give any opinion of the work, as to style of which you are infinitely a better judge than I am, but as to fact can say it is correct according to the best information at present to be had here. I am desired by Mr. Charless to say that should the book fail of purchasers it will not create in him the feelings of a disappointed man; and that he only wishes you to do with it what your own Judgment and known Philanthropy may prompt you to think best. If you meet with an *honest* Bookseller who will purchase, he has only to make up the value in Types of any kind from Long Primes to English and ship them in a Box to Kenner & Henderson Orleans address for Mr. Jos Charless and Types part worn will answer for this remote part of the world.

Mr. Charles desires me also to say that this will be accompanied by some few articles of Indian Costume and Nat History which he offers for your acceptance, not as worthy by their intrinsic value but as adding perhaps a mite to the stock of Human Knowledge.

I am Sir your Very
obed Servant
John Bradbury

I go on my Missouri expedition tomorrow and shall meet the Boat then at St. Charles on that river she sailed about an hour ago.[15]

[15] In Bradbury's *Travels* this date is given as March 12. Irving in *Astoria* is silent on this point.

17. JOHN LEIGH BRADBURY TO WILLIAM ROSCOE

Birmingham Mar. 18 1811

Wm. Roscoe Esqr.
Sir.

I have heard from London since I had the honour of addressing you and I am much afraid that nothing can be done for my Father and consequently I cannot join him. I took the liberty of enquiring if any thing had been yet received from my Father and of asking a little of the Wool, I am sorry to give you so much trouble. I came to this town to try some experiments relative to the business I took the liberty of mentioning to you and shall return to Gloucester in a few days.

I am Sir with respect
your obliged servant
John Leigh Bradbury

Mr. Charles Weavers
Gloucester.

The two following letters complete the series written by Bradbury. Both deal with the journey up the Missouri, one having been written from points on the way up, the other from St. Louis after Bradbury had returned there.

Bradbury began his first letter at the mouth of the Nodoway (the English version of the word "Naduet"), near where St. Joseph, Missouri, is now situated. At this spot the main body of Hunt's expedition had wintered. He was disappointed in what he saw, the vegetation having changed little from that near St. Louis.

It is interesting that the letters do not mention Nuttall, who was of course in the same party. In the *Travels* (p. 22) we read of a walk with Nuttall on which the latter occasioned Bradbury some inconvenience by not knowing how to swim. The reference to "scrapes" in the letter doubtless originated in such adventures. Later, apparently, the two naturalists went their own ways—one destined for immortality (at least of the botanical kind), the other for comparative obscurity. Bradbury covered long stretches of the country on foot, leaving the boat in the morning and joining it again in the evening.

A later installment of the same letter is dated from the "Mahas village," some eighty miles north of the situation of the present Omaha, Nebraska. By this Bradbury was delighting in an entirely new country and a new flora. His overland journey on foot to the Otto village on the Platte is described in detail in the *Travels* (p. 51–65). A number of his extant specimens are labeled "Otto nation" or "Otto village" or refer to "La Rivière

Platte"; these were evidently among those sent back from the Maha village [this is not mentioned, however, in the *Travels,* where (p. 65) he speaks only of forwarding letters back to the United States by Mr. James Aird, an old trader].

The last letter is concerned partly with Indians and the danger to be apprehended from them. He had himself been in great danger at least once —he tells the story on page 75 of the *Travels,* and Irving retells it on page 136 of *Astoria.* The account of the "medecine" of the unsuccessful war party is also based on a real incident which occurred during Bradbury's absence from the party on his journey to the Ottoes (*Travels,* p. 70).

18. JOHN BRADBURY TO WILLIAM ROSCOE

Mouth of the River Naduet 19 Ap 1811

Sir.

Four Days before I left St. Louis I received your much esteemed favor of the 30th Octr. It was inclosed in one from Dr. Dowe together with one from Mr. Sheppard, the Docr. also informed me that I was empowered to draw on him for 20 Pounds. . . . I wrote in reply *attempting* to thank you Sir, but having previously given up the expectation of my Post arriving at St. Louis before my departure I had already written to you, and by a mistake unaccountable to me I dispatched the wrong Letter, this I did not discover untill we had passed beyond the Settlements. Let this Sir convey my warmest acknowledgments for your bounty to which you will perceive, I shall probably be indebted for the preservation of my existance.

The specimen of wool I sent you is as you Sir suggest undoubtedly from *Bos Moschatus.* I called it the Buffaloe as adopting the popular name. I have already seen it or the carcases altho we are not yet arrived at the Buffaloe country. In their migrations they go in herds of 10, 20 or even perhaps 100.000, one of these herds in passing over the Ice of the Missouri above this place about 6 weeks ago broke in and it is supposed as many as 1000 perished, we have met the carcases floating down every day. I think I mentioned in my former that I go along with a party who are on their way to the *Pacific Ocean*; how far I shall proceed with them I cannot as yet determine, but trust that I shall be governed by a sense of what I conceive to be my duty, unless I find the natural productions in the vicinity of this River change materially from what they are here I purpose when we arrive at the Rikaree Nation to purchase Horses, hire a Hunter to accompany me and cross Southward to the *Arkansas* a distance of about 400 Miles at present the Experience I have had of the Plants since I left St. Louis inclines me to think I must adopt this plan those in the Vicinity of the River are exactly the same here as they are 500 Miles below. I am told that 5 or 600 Miles farther up the soil and its productions entirely change. Should I find that to be truly the case I shall remain during the summer on this River either with some nation of Indians or alone as may hereafter appear more expedient: Having become tolerably expert with the Rifle I have no doubt of being able to subsist myself in the woods and shall therefore probably adopt the latter plan. For the reason I have mentioned I have little to communicate as yet in respect to plants, but indeed I have not had much opportunity as the weather has been excessively bad, by continued rain: two species of Fern for which I can find no discription and a white Erythronium (which I have before mentioned) are all I have marked out for sending since I commenced my voyage. I have a species of *Talpa* which will probably be new it agrees with the European Mole in having five toes on each Foot but is much smaller and has a beautiful silvery gloss.

It is perhaps right to mention for the satisfaction of the Committee that I remain in perfect health altho the bad weather has been much against me, as also has some Scrapes I have got into in my walks, from which I could only extricate myself by swimming.

I have no opportunity of dispatching this Letter now ,but we expect to meet a Fur Boat which will afford one. I shall therefore leave it unclosed that I may have an opportunity of saying more if necessary.

Mahas Village 350 miles above St. Louis 12 May

Not having had an opportunity of sending this I have to add that I have now sanguine hopes of doing enough on this River to satisfy those who patronize me and discharge my duty. from the mouth of the River termed by the French *La Rivière Platte* the soil and its productions have materially changed and I send to St. Louis to be forwarded to you Sir for the Committee specimens of 25 Plants for which I can find no discription three of them may probably be valuable say [?] a Yucca a Cherry and an Anemone. I hope you will receive them before this arrives as they will go by a shorter route.

Many of these have been collected in a Journey by Land which I undertook along with an Indian Trader one of the Partners in this expedition who had formerly traded with the Otto Indians When on the Platte River with them he had business and we quitted the Boats 200 miles below this place by water went to the Ottoes 40 miles & from them to this place 120 across the country.

If possible Sir you shall hear from me again during the summer if not I beg you will believe that nothing shall be wanting on my part to discharge faithfully the trust reposed in me

I am Sir your grateful
Obed Servant
John Bradbury

Since I made up the Packet of specimens I think I have forgot to give the discription of the Shrub in Dioecia octandria.

Male Calyx 4 parted
Corol none
Stamens Eight Short anthers simple
Female—*Calyx* 4 Parted
Corol none
Pistm. Stigma capitate
Stile short
Germ—becoming a one seeded berry

19. JOHN BRADBURY TO WILLIAM ROSCOE

Merion (?) Nr. St. Louis 16 Augt. 1811

Sir.

I arrived here a few Days ago from my excursion up the Missouri which I ascended to the *Gros Ventre* Nation of Indians 1650 Miles from this place. the Sioux Indians assembled as we expected to oppose us but on our going up to them they declined to engage. I wrote to you Sir from the Mahas Nation & sent a small Packet of specimens in which I made some mistakes the common consequence of hasty communications. the Shrub is *Hippophae canadinsis* I found none but withered male flowers unexpanded on dissecting them the anthers split in two, the Yucca is probably *Y. Gloriosa* if so the . . . deceived me.

I found the country & its productions gradually to change as we proceeded up the River and anxious to secure in quantities those Plants which besides being as I judged new were beautiful I bought all the empty *caissettes* or trunks belonging to the Canadian Boatmen & an old Barge belonging the Party who left the River to go by Land—I was enabled by this means to bring down from 4 to 5000 Roots planted in 17 Trunks 4/5 of these are now dead as the Situation in which I found myself on my arrival here obliged me to look out for *Immediate* employment of another nature which rendered it impossible for me to take care of them.

I am more than ever confirmed in the opinion that the Western parts of this country are more abundant in unknown Plants than any part of the Globe the country of the Amazons perhaps excepted & I do not dispair of being able to visit them in some years by my own means, the most dangerous part being in some measure accomplished: none of the Indians from the South west bank of the Missouri to the Gulf of Mexico being hostile to the Whites and there is little danger in meeting them excepting when they are returning from War unsuccessful When they sometimes throw away their cloaths and make it *their Medicine* (a Vow) to kill all they meet and even then if you have obliged any one of the party formerly they can save you even a Squaw as they hold private friendship sacred. I have availed myself of this trait in the Indian character and made Presents to some of the chief Warriors in each Nation I have passed through. I had also arranged to accompany the Osages next summer who go annually for Salt to the great Salt lick beyond the Arkansas on the borders of Mexico.

I now find Sir that the wool I sent you was from Bos Americanus having had frequent opportunities of examining the Animal. Bos Moschatus I do not think is found on the Missouri. I am of opinion that this wool might become an Article in Manufacture and that great quantities might be obtained from the Indians at an easy rate as it requires much less labour to shear a skin than to dress it, and for a dressed Buffaloe Skin the usual price to the Indians is 20 Musquit balls & 20 loads of Powder.

Within is a note to the Committee of the Garden which I inclose as not knowing how else to address them. I fancy Sir to leave it at the Garden will be the least troublesome to you.

I am Sir with the most grateful
respect your Very obed Servant
John Bradbury

The nature of the "note to the Committee of the Garden" becomes clear in a letter from Roscoe (no. 20 below); it was a resignation from his appointment, to take effect at once although his stipend had been paid until the following February. On the exact nature of the difference between Bradbury and the authorities at the Botanic Garden I can shed no light. It is evident from several of the letters that Bradbury's plans were constantly thwarted by lack of money; the amount agreed upon was obviously insufficient for his purposes. On the one hand Bradbury doubtless felt that the Committee should be more generous; on the other, the gentlemen in Liverpool must have felt disappointed at the paucity of material return from their outlay. In the *Travels* (p. 191) Bradbury writes of "those who had determined to withhold" his remittance from the Garden; they finally relented, whoever they were, and the remittance was received in St. Louis near the end of November, 1811. At the same time he received a letter from the "person who managed the Botanic Garden at Liverpool" informing him that his former collection had been received, more than one thousand plants had been potted, and many seeds were germinating. The disparaging tone of his reference to Shepherd (the "person" in question) suggests that he was one of those who were bent on impeding Bradbury's work—or perhaps on penalizing him for the lack of what they considered satisfactory results.

Several particulars in the *Travels* do not square very well with the account in this letter. According to the former, Hunt gave Bradbury the barge for his return voyage. Manuel Lisa, representing a rival fur company, purchased the other three boats, and arranged to accompany Bradbury and allow him time to collect, in exchange for his boat (p. 171). Lisa did not live up to his part of the agreement, and the journey down stream was made almost without a stop, to Bradbury's intense disappointment.

About ten days after his return to St. Louis (near the beginning of August), according to the *Travels,* Bradbury was taken ill and confined more or less until December (p. 190); nothing is said of the collections. The account in the letter says nothing of illness but introduces the necessity of seeking other employment as an explanation for the damage to his collections. Since the letter was written at the time and the book five or six years later, the former is intrinsically more credible.

Bradbury's collections from the Missouri seem to have reached Roscoe through his son, who offers them in the third letter following. He also had apparently received a full account of his father's misfortunes.

20. JOHN LEIGH BRADBURY TO WILLIAM ROSCOE

Wm. Roscoe Esqr.
Sir.

In the last letter I had the honour of receiving from you, you had the goodness to promise to communicate any matters that might occur in regard to my Father on learning my adress.

I am anxious to know if the society still patronize him, or if any thing has been received from him. By letters received by a friend of his in London dated May 1811 he was *850* Miles beyond St. Louis, in good health and spirits, & successful. I am much afraid he has not the least chance of support in London.

I am Sir with particular
Respect
John Leigh Bradbury
at Charles Weavers Esqr.
Gloucester.
16 Oct. 1811

21. WILLIAM ROSCOE TO JOHN LEIGH BRADBURY

Allerton 26th decr. 1811

To J.L. Bradbury
Sir.

As you desired to be informed when I heard from your father I take this opportunity of acquainting you that I lately recd. a letter from him dated at Merino near St. Louis 16 Augt. last when he had just arrived from his Excursion up the Missouri—At the same time he inclosed a note to the Committee of the Botanic Garden, informing them that he had sent to them all the plants which they were under their Agreement entitled to, & that what he was now collecting was for his own use.

I scarcely need to say that this communication has given great dissatisfaction to the Committee, who have regularly remitted to Mr. Bradbury according to his order his annual payment of £100 p. an: for 3 years & which 3 years will not expire till the end of the present; consequently whatever may be collected till that period is under the contract, to be sent to the Liverpool Botanic Garden for the purposes therein specified.

This is the more dissatisfactory to the Committee as the Plants already received are considered by the Committee as by no means adequate to the Payments they have made to Mr. Bradbury, & they cannot therefore but conceive that even if the last £100 had not been remitted they ought to have recd. some considerable addn. as a remuneration for their former Paymts. I state these matters to you that if you have an opportunity, you may acquaint your father with them & I am the more desirous of this as I feel myself anxious that he shd. acquit himself with honor & punctuality of an engagement which I was the chief means of obtaining for him, & which I shall be happy to hear may lead to something to his greater advantage.

Some months since I had a Letter from Mrs. Bradbury complaining of her pecuniary inconvenience, in consequence of which I remitted her a bank note for ten pounds as a present on my own acct. & as a proof of my confidence in Mr. Bradbury's good conduct. Having not heard of its being recd. I fear my letter to Ireland may have miscarried & shd. be glad you wd. ask the question when you write, being Sir

Your very faithful & obt. Sert.
W. Roscoe
To Mr. J.L. Bradbury
at
Chas. Weaver's Esqr.
Gloucester

22. JOHN LEIGH BRADBURY TO WILLIAM ROSCOE

Charles Weavers Esqr. Gloucester
Dec. 29. 1811

W. Roscoe Esqr.
Sir

I had the pleasure of receiving your letter and previously the mortification of one from my Father mentioning having resigned the Patronage of the Liverpool Society. I cannot account for such a step except on the supposition that he thought the term expired; or from a pique he might have felt on my

answering him he had little chance of a continuance of their support when his engagements were out. Whatever may have induced a measure of which none of his friends approve, I venture to pledge myself that he will do any thing further to the satisfaction of the Society which you or they think proper, and if he does not, I will. of this I beg you Sir to assure the Society and request them at all events to suspend their judgment till I again hear from him.

I hope he will never number among his misfortunes, the loss of your good opinion for with it must be lost the good opinion of the world. I should consider the man contemptable who would sacrifice your friendship for any advantage; and I cannot conceive that any man would do it where an advantage apears.

It is possible that you and the Society may suppose from my Fathers letter and knowing the previous situation of his family that he has some better engagement in view. The fact is he has none. He wrote on the Banks of the Missouri without a Shilling in his Pocket, and every thing stolen by the Indians except the cloths on his back & his Plants. In this situation he also wrote to me, inclosing specimens of Plants brought *1650* Miles down the River, and desired me to form a new connexion and shew the plants. I have wrote to Mr. Buchanan but shall not now attempt any thing further: I have not shewn the Plants, and I will send them if you please to you I will write to my Mother on the subject you mention and I cannot express my sorrow that you should have so much Trouble with us. I am Sir with respect and gratitude your obliged ser.

J. L. Bradbury.

23. WILLIAM BULLOCK TO WILLIAM ROSCOE

London 4 Novr. 1813.

Sir

I laid your Letter before Sir Jh. Banks who requested me to return his warmest thanks for the very liberal manner in which you have permitted him to take a specimen of each of the duplicates of the Plants sent by Bradbury and that he will consider it his duty and have great pleasure in forwarding to the Liverpool Garden whatever is in his power to add to it. Mr. Brown will make the selection tomorrow when they together with Dr. Roxboroughs shall be forwarded as you request.

Mr. Lambert has just called on me to request me to make a similar request to you on his part that I did for Sr. Josh. which I do only because I promised him not having the same reasons that I had in the other Case he *says* he has 20 new Peruvian Plants living that he will send you in return you will please to determine for yourself in this case.—I would certainly select the duplicates myself were the case mine I am Sir very truly yours

W. Bullock

Here the record ends. Bradbury, cheated of his scientific and horticultural ambitions, was an embittered and disappointed man. We know from other sources[16] that he went from St. Louis to New Orleans and thence to New York, where he was engaged in business for several years, with little pleasure and small financial gain, and still dreaming of his darling object, the exploration of the Arkansas. Finally in 1816 he made his way back to Liverpool, where he published his *Travels*. This is said to have exhausted his slender means; it is a pity he could not have received the sums paid now for single copies. We know also that he gathered up his wife and "numerous family" and with them left England forever in 1817 or 1818; coming first to St. Louis, where he stayed at least until 1821; then moving to Middletown, Kentucky, where he died in 1823.

Bradbury was defeated in the great undertaking of his life chiefly by lack of appreciation (on his part and on that of his sponsors) of the difficulties of the task. To travel the American wilderness and bring back living plants from it required more money than had been foreseen or was forthcoming; though Bradbury himself soon realized this, he was unable to "sell" the idea to his patrons in Liverpool. This was unfortunate; probably a few additional hundreds of pounds would have yielded results gratifying to all concerned. As for Bradbury's scientific endeavor, we can only speculate on what he would have accomplished had not his collection been turned over to Lambert and Pursh. It may be suspected that he lacked the scientific acumen of either Pursh or Nuttall, and that the actual outcome, while distressing to Bradbury, was fortunate for posterity. On the other hand it is evident that he recognized the novelty of many of the species which he collected, and, given more means, could perhaps have described them properly. Whatever be the truth in this, we may share his just resentment at the circumstances which deprived him even of the chance to show what he could do. He seems never to have attempted botanical work again.

II. THE SPECIMENS

During Bradbury's absence Pursh published his *Flora* (1814), and in it the new species based on Bradbury's collections. Bradbury comments on this bitterly in the *Travels:*

[16] *Proc. Amer. Philos. Soc.* **68**: 142–149, 1929. Spaulding, Perley, A biographical history of botany at St. Louis, *Pop. Sci. Mo.* **73**: 493–495, 1908.

I found that my design [of describing the collections] was frustrated, by my collection having been submitted to the inspection of a person of the name of Pursh, who has published the most interesting of my plants in an appendix to the *Flora Americae septentrionalis.*

And again:

This man has been suffered to examine the collection of specimens which I sent to Liverpool, and to describe almost the whole, thereby depriving me both of the credit and profit of what was justly due to me.[17]

Having failed in most of his efforts to transmit living American plants to England, he now saw himself deprived of the opportunity for making a scientific contribution.

The truth about the history of the specimens may be fairly clearly discerned in the letters cited above. It is plain that (except for a few sent from St. Louis and from the Nodoway in January and April, 1811) most of the specimens were sent to John Leigh Bradbury in December, 1811. The latter made little or no attempt to follow his father's instructions to sell them, but sent them to Roscoe in Liverpool. At the Botanic Garden they were presumably sorted over, and duplicates were sent to various persons in exchange for their collections. It must have been in this way that Thomas Taylor obtained those which he later gave to Sir William Jackson Hooker; these are the specimens preserved in the Hooker Herbarium at Kew.[18] Some of the duplicates reached Sir Joseph Banks in London, and at the instance of Bullock a set was sent to Aylmer Bourke Lambert. Lambert annotated them mostly with the phrase "Louisiana. Bradbury." Pursh had access to Lambert's herbarium, and on these specimens founded new species, besides determining such as represented species already known.[19] At the sale of Lambert's herbarium in 1842, most of the North American specimens were purchased apparently by Dr. Thomas B. Wilson and by him presented to the Academy of Natural Sciences of Philadelphia.[20] In that institution they are still preserved, with Lambert's and Pursh's annotations to be seen upon them. The types, therefore, of Pursh's species are the duplicates used for exchange by Roscoe.

The specimens which remained in Liverpool—the "originals" as contrasted with the duplicates—were seen in 1839 by Thomas Batt Hall.[21] The collection of the Botanic Garden was transferred in 1909 to the Liverpool Public Museums. It consisted of 40,000 specimens, unfortunately extensively damaged by insects and fungi. Some 16,000 were retained, presumably including the Bradbury plants.[22] When the latter were sought in 1934 they could not be found; but they were discovered subsequently and reinstated in the herbarium. During the summer of 1948 I was privileged, through the courtesy of Mr. Stansfield and the officers of the Herbarium at Kew, to study these specimens. Later I visited the Academy of Natural Sciences of Philadelphia, and with the kind cooperation of Dr. Pennell examined the Bradbury specimens there.

The collection at Liverpool comprises about 115 unmounted specimens which are more or less determinable. Some are in surprisingly good condition, considering that they were carried across the prairies, protected from torrential rains by being lain upon (*Travels,* p. 61), and saved from the depredations of the Indians who got away with most of Bradbury's belongings (letter no. 22, above). Some show the expected wear and tear, and some are mere fragments. Many bear Bradbury's field labels, with his notes and attempts at determination, and the locality. John Shepherd added his own ideas of both on slips of blue paper. Many lack one or both of these labels. (Exact localities are missing from the types at Philadelphia, which can be assigned to more or less definite places of collection only by comparison with the isotypes at Liverpool and Kew.) Some specimens are obviously included by error, being European plants (nos. 13, 14, 17 in the list which follows). It is evident also that not all the plants came from the Missouri (see, for example, nos. 41, 75, 79). Some are not even so labeled by Shepherd, so that the collection is obviously something of a miscellany. The information on

[17] *Travels,* vii and 79. This seems to be clear evidence that not only was he not consulted about the disposition or use of his plants, but did not even see a copy of Pursh's work until nearly three years after its publication.

[18] Hooker cited some of them in the *Flora Boreali-Americana* as early as 1829.

[19] In his preface Pursh says "I am also highly indebted to William Roscoe, Esq., who very obligingly communicated to me Mr. Bradbury's Plants collected in Upper Louisiana" (*Flora Americae septentrionalis,* p. xvii). This is in conflict with the story outlined above; but there is no doubt that the specimens annotated by Pursh also carried Lambert's handwriting. Perhaps it was at Pursh's request that Roscoe sent the plants to Lambert.

[20] For this information I am indebted to Dr. F. W. Pennell.

[21] *Naturalist* **4**: 397, 1839.

[22] *Handbook and guide to the herbarium collections in the Public Museums,* 39, Liverpool, 1935.

the labels must therefore be regarded with scepticism. The same is true of the determinations which follow below. Obviously the precise identity of some of these specimens—even were they less fragmentary—is a matter for the specialist. Many, on the other hand, can be named with fair assurance.[23]

The specimens at Philadelphia are, as already mentioned, annotated by Lambert and by Pursh; one bears also a note in Bradbury's hand.

The specimens at Kew are enumerated in *Kew Bull.* **1934**: 54–61.

The annotated list which follows includes the Bradbury specimens in all three of these collections, in an effort to bring together a complete list of his extant specimens. The place of deposit is indicated by the letter in parenthesis which follows the number: L for Liverpool, K for Kew, P for Philadelphia. The quoted statements are Bradbury's notes on his labels, unless attributed to another.

1. (L) Cheilanthes feei Moore, *Ind. Fil.* xxxviii. 1857.

Myriopteris gracilis Fée, *Gen. Fil.* 150. 1857. Not *Cheilanthes gracilis* Kaulf. 1824.

"Is doubtless an Acrostichum but not apparently described. Rocks Missouri."

In the *Travels*, p. 338, Bradbury mentions "*Cheilanthes Dealbata* and *Vestita*, Manitou rocks on the Missouri." The latter is presumably *Cheilanthes feei*, common along the bluffs in Missouri, and represented in Liverpool by a minute fragment. The former, *Notholaena dealbata* (Pursh) Kunze, is of special interest because of the rarity of this fern in Missouri. It was rediscovered by Francis Potter Daniels near Rocheport, Missouri (*Univ. Mo. Stud. Sci.* **1**[2]: 80, 1907), and more recently in the same place by other students. This is almost certainly the place visited by Bradbury in 1811; Manitou Creek enters the Missouri at Rocheport, at the foot of lofty and picturesque bluffs.

2. (L) Osmunda regalis L. *Sp. Pl.* 1065. 1753.

"Is this Osmunda regalis? Spikes only decompound. No. 113."

3. (L) Cenchrus pauciflorus Benth. *Bot. Voy. Sulph.* 56. 1844.

"This Grass is most probably Cenchrus tribuloides but badly discribed as the spike is simple and the flowers Polygamous."

4. (K) Aristida longiseta Steud. *Syn. Gram.* 420. 1854.

Aristida pallens sensu Pursh, *Fl. Am. Sept.* 728. 1814. Not *Aristida pallens* Cav. 1799.

"Mandan country."

In the *Travels*, p. 335, this is listed from "Hills on the Merrimac" [near St. Louis].

5. (L) Eleusine indica (L.) Gaertn. *Fruct.* **1**: 8. 1788.

Cynosurus indicus L. *Sp. Pl.* 72. 1753.

"*Cynosurus.* Is most likely discribed but find it difficult to ascertain—Culm compressed procumbent. leaves with a few scattered erect hairs. Calyx 4 or 5 flowered Spikes mostly 4 often with a lateral one."

In the *Travels*, p. 335, *Cynosurus secundus* is listed from "Mississippi bluffs." It seems possible that this specimen is *Cynosurus secundus* Pursh (*Fl. Am. Sept.* 728. 1814); but Hitchcock (*Man. Grasses U. S.* 806) identifies *C. secundus* with *Bouteloua curtipendula* (Michx.) Torrey.

The existence of this introduced weed in Missouri in 1810 is of course questionable.

6. (L) Panicum capillare L. *Sp. Pl.* 58. 1753.

"Panicum. Does not agree with *diffusum* as that is said to grow tall, this does not exceed 3 feet in heighth none of the other discriptions will in any way apply—I suspect that the discription of diffusum is erroneous in that point—this is a very comm. grass in America. Can it be Trichodes laxifolium [*Trichodium laxiflorum*] of Michaux."

7. (K) Oryzopsis hymenoides (R. & S.) Ricker; Piper, *Contr. U. S. Nat. Herb.* **11**: 109. 1906.

Stipa membranacea Pursh, *Fl. Am. Sept.* 728. 1814. Not *Stipa membranacea* L. 1753.

Stipa hymenoides R. & S. *Syst.* **2**: 339. 1817.

"Prairies, Mandan." The locality is given as Fort Mandan [North Dakota] in the *Travels*, p. 335; Bradbury adds: "*Probably* not a *Stipa*."

8. (L) Panicum villosissimum Nash, *Bull. Torrey Club* **23**: 149. 1896. "Panicum Spec. Nov. No. 128."

9. (L) Tripsacum dactyloides L. *Syst. Nat.* ed. 10. **2**: 1261. 1759.

Coix dactyloides L. *Sp. Pl.* 972. 1753.

"This grass is of *Monoecia triandria* and differs from any Genus in that order discribed in Sys. Nat. (male) *Calyx* of two glumes, one membranaceous containing two florets—Corol of two membranaceous glumes.

(female) Calyx of one coriaceous glume containing one Floret. *Corol.* of four membranaceous glumes. Obs. Surely is not a Tripsacum (resembling Triticum). Spike compound, Hermaphrodite—Female lodged in a cavity."

[23] A number of the more interesting of the specimens deposited at Liverpool have been photographed and prints are available to libraries and herbaria.

10. (L) Uniola latifolia Michx. *Fl. Bor.-Am.* **1**: 70. 1803.

"Triandria digynia. This elegant grass is certainly not reducible to any Genus in the Systema Natura, but as I have seen it in different parts West of the Alleghenies probably it did not escape Michaux, to whose Industry and accuracy I shall always bear witness."

11. (L) Carex ? artitecta[24] Mackenzie, *N. Am. Fl.* **18**: 189. 1935.

"Carex pilulifera Le Haut Missouri."

According to Mackenzie the northern limit of this species is in Iowa.

12. (L) Carex canescens L. *Sp. Pl.* 974. 1753.

Carex buxbaumii Wahl. *Sv. Vet.-Akad. Nya Handl.* **24**: 163. 1803.

"Carex I find no discription of this species. No. 121."

13. (L) Carex depauperata Gooden. *Trans. Linn. Soc.* **2**: 181. 1794.

Carex ventricosa Curt. *Fl. Lond.* **6**: pl. 68. 1790.

"*Carex ventricosa* 2 June 1807." Obviously not a part of the American collections.

14. (L) Carex disticha Huds. *Fl. Angl.* 347. 1762.

"Carex distycha? 2 June 1807."

See the note on the preceding (no. 13).

15. (L) Carex foenea Willd. *Enum.* 957.

Carex siccata Dewey, *Amer. Jour. Sci.* **10**: 278, 1826. See Svenson, *Rhodora* **40**: 325, 1938.

"C. Arenaria ? near the Otto village. La Rivière Platte [Nebraska]."

16. (L) Carex meadii Dewey, *Amer. Jour. Sci.* **43**: 90. 1842.

"C. Panicea Le Haut Missouri."

17. (L) Carex paniculata L. *Cent. Pl.* **1**: 32, 1755.

"Carex Paniculata 2 June 1807."

See the note on no. 13.

18. (L) Carex sprengelii Dewey; Spreng. *Syst.* **3**: 827. 1826.

"Has some resemblance to *Carex sylvatica* but is another plant not known to me. Woods Missouri."

19. (L) Dulichium arundinaceum (L.) Britton, *Bull. Torrey Club* **21**: 29. 1894.

Cyperus arundinacea L. *Sp. Pl.* 44. 1753.

"Cyperus ? Culm round jointed thick terminal leafy. spikelets frondose—Seeds with 8 awns from the base. No. 95."

[24] The species of Cyperaceae were kindly determined by Mr. E. Nelmes.

20. (L) Scleria ? triglomerata Michx. *Fl. Bor. Am.* **2**: 168. 1803.

"This singular species of Carex grows in the Priaries & is of a compleatly dry habit. No. 63."

Probably of the 1810 collection from eastern Missouri.

21. (L) Heteranthera limosa (Sw.) Willd. *Ges. Nat. Freunde Berlin Neue Schr.* **3**: 439. 1801.

Pontederia limosa Sw. Prodr. 57. 1788.

In unidentified hands: "Leptanthus ovalis LBG 1814." "Missouri Bradbury."

22. (L) Lilium ? canadense L. *Sp. Pl.* 303. 1753.

In an unidentified hand: "Lilium Carolinianum Bradbury L.B.G. Sept. 1814."

23. (L) Melanthium virginicum L. *Sp. Pl.* 339. 1753.

In an unidentified hand: "Melanthium virginicum Bradbury. L.B.G. 1813."

24. (L) Yucca ? angustifolia Pursh, *Fl. Am. Sept.* 227. 1814.

"I suppose a Yucca. High bare bluffs near the Mouth of La Rivière Platte."

This is doubtless the plant referred to in the *Travels*, p. 336: "Yucca angustifolia, Missouri Bluffs, opposite the mouth of Papillon Creek." See also letters no. 18 (the part written from the "Mahas village") and no. 19. Pursh described the species from a collection by Nuttall, "on the banks of the Missouri," very probably made at the same time as this.

The collection is too fragmentary to be determined with certainty.

25. (L) Cypripedium sp.

"Cypripedium. Can this have been mistaken for C. Calceolus? Flowers odorous."

See letter no. 7.

26. (L) Cypripedium sp.

In an unidentified hand: "Cypripedium pubescens. No. 1 LBG. 1820 No. 2 Bradbury 3 Smith." Only two plants found.

27. (L) Salix nigra Marsh. *Arbust.* 139. 1785.

"Salix. Hexandrous Islands of the Platte [Nebraska] Tree."

28. (L) Quercus marilandica Muench. *Hausvater* **5**: 253. 1770.

"Quercus nigra does this agree with your Q. nigra as the Black Oak is Q. tinctoria. This is Black Jack."

29. (P) Rumex venosus Pursh, *Fl. Am. Sept.* 733. 1814.

Annotated by Lambert "Louisiana Bradbury" and by Pursh "Rumex venosus." TYPE.

(L) Isotype.

"Rumex. Flowers monoecious stem procumbent, spikes lateral leaves ovate pointed at both ends glaucous sheaths & seed vessels large. Sides of the Corn du Cerf [Nebraska]." In the *Travels*, p. 336, listed from "Big bend, Missouri." This refers to a well known bend in the river in what is now South Dakota (see also the *Travels*, p. 176).

30. (L) Chenopodium sp.

"Chenopodium (Bradbury)."

31. (P) Oxybaphus hirsutus (Pursh) Choisy; DC. *Prodr.* **13**[2]: 433. 1849.

Allionia hirsuta Pursh. *Fl. Am. Sept.* 728. 1814.

Annotated by Lambert: "Louisiana. Bradbery." Type.

(L) Isotype.

Annotated by Shepherd: "Allionia hirsuta Pursh Flora America Sept. V. 2. P. 728. Bradbury Habitat in Upper Louisiana. 1810."

(K) Isotype.

"le haut Missouri." According to the *Travels*, p. 335, found on "bluffs near the Aricara village [South Dakota]." Nuttall (*Gen.* **1**: 26. 1818) mentions the same place. This is in conflict with the "1810" on Shepherd's label, which would refer to plants gathered near St. Louis.

32. (P) Oxybaphus linearis (Pursh) B.L. Robinson, *Rhodora* **10**: 31. 1908.

Allionia linearis Pursh, *Fl. Am. Sept.* 728. 1814.

Annotated by Lambert: "Louisiana Bradery" and by Pursh: "Allionia angustifolia." Type.

(L) Isotype. Annotated by Shepherd: "Allionia linearis Pursh Flora Americ-Sept. V. 2. p. 728. Bradbury. Habitat in Upper Louisiana, 1810." According to the *Travels*, p. 335, from "bluffs near the Aricara village." *Cf.* comment on no. 31.

33. (P) Oxybaphus nyctagineus (Michx.) Sweet, *Hort. Brit.* 334. 1826.

Allionia nyctaginea Michx. *Fl. Bor.-Am.* **1**: 100. 1803.

Allionia ovata Pursh, *Fl. Am. Sept.* 97. 1814.

The specimen is annotated by Lambert: "1. America Sept: Herb: Lewis & Clark. 2. Louisiana. Bradbery." And by Pursh: "Allionia albida ? Walt. Open plains. Septbr:1. 1804." No. 1 is the Type of *Allionia ovata* Pursh.

(L) Annotated by Shepherd: "Allionia nyctaginea Pursh Flora America. Septents. V. 1. P. 47. Calymenia Nyctaginea Bradbury. Habitat on the banks of the River Tennassee. N. America. 1810." There is no evidence that Bradbury was ever on the Tennessee; certainly not in 1810. The label seems to have originated in a confusion with Michaux' description; his type was collected "ad ripas fluminis Tennassee." According to the *Travels*, p. 335, Bradbury's specimen came from the "banks of the Missouri, above the Big Bend [South Dakota]."

34. (L) Anemone canadensis L. *Syst. Nat.* ed. 12. **3**: 231. 1768.

"Anemone Banks Mississippi No. 75."

35. (L) Anemone caroliniana Walt. *Fl. Car.* 157. 1788.

"This male Anemone does not seem to be a described species—near the Otto Village La Rivière Platte [Nebraska]."

36. (L) Pulsatilla hirsutissima (Pursh) Britton, *Ann. N. Y. Acad.* **6**: 217. 1891.

Clematis hirsutissima Pursh. *Fl. Am. Sept.* 385. 1814.

Anemone ludoviciana Nutt. *Gen.* **2**: 20. 1818.

"This fine Anemone seems to resemble A. Pulsatilla flowers light blue. Abundance on the highest bluffs above the Maha Village [Nebraska]."

Clematis hirsutissima Pursh was described from the "plains of the Columbia River." Nuttall's name was based on his own collections made on the Missouri above the Platte.

37. (P) Ranunculus flabellaris Raf. *Am. Mo. Mag.* **2**: 344. Mr. 1818.

Ranunculus multifidus Pursh, *Fl. Am. Sept.* 736. 1814. Not *R. multifidus* Forssk. 1775.

Ranunculus delphinifolius Torr.; Eaton, *Man.* ed. 2. 395. My or later 1818.

Annotated by Lambert: "[Louisi]ana. Bradbury."[25] and by Pursh: "Ranunculus multifidus." Type of *R. multifidus* Pursh.

(L) Isotype.

"This species of Ranunculus differs from any I have ever examined the nectary being inverted from the order in which it is usually placed in the Genus in this it is a hollow cup with the mouth turned from the Petal—Leaves all dissected. Marshes near the Platte."

In the *Travels*, p. 337, listed "in stagnant pools near the Sepulchre bluffs." This landmark is described on pp. 63 and 64; it was a bluff crowned by a large pile of rocks which served as a monument to the famous (or infamous) chief Blackbird of the Ottoes. Bradbury passed it on his return from the Otto village on the Platte, on 9 May 1811. Probably all three of the extant specimens were gathered at the same time, so that this can be cited with some assurance as the type locality of *R. multifidus* Pursh.

(K) Isotype.

"Otto Nation."

[25] Many of the specimens in the Academy of Natural Sciences of Philadelphia have been remounted, and parts of the old labels have been sometimes cut off in the process.

38. (L) Viorna fremontii (S. Wats.) Heller, *Muhlenbergia* **5**: 96. 1910.

Clematis fremontii S. Wats. *Proc. Am. Acad.* **10**: 339. 1875.

"This Plant I did not find untill the seeds were ripe but no doubt remains with me that it is a Clematis—*Erect*, leaves simple, ovate, *smooth* strongly veined entire. Plant rigid and rustling to the touch—Inform me if it is *C. ochroleuca*. Found 5 August 1810. Flint Hills betwixt the Merrimack River & the Mines [eastern Missouri]."

In Shepherd's hand: "Clematis ochroleuca Willd. Sp. Pl. P. 1294. Habitat in America."

Palmer and Steyermark (*Ann. Mo. Bot. Gard.* **22**: 542. 1934) list this species from "Limestone glades . . . Eastern Mo., south of the Missouri River."

39. (K) Erysimum asperum (Nutt.) DC. *Syst.* **2**: 505. 1821.

Erysimum lanceolatum Pursh, *Fl. Am. Sept.* 436. 1814. Not *Erysimum lanceolatum* R. Br. 1812.
Cheiranthus asper Nutt. *Gen.* **2**: 69. 1818.

"1100 miles up the Missouri [South Dakota]." Listed in the *Travels*, p. 337, as *"Erysimum Lanceolatum* or *Cheiranthus Erysimoides*, a connecting link between Erysimum and Cheiranthus, used as medicine by the Aricaras." The species is mentioned also on p. 116 as forming a large part of the ingredients of an Aricara medicine bag.

40. (L) Lesquerella ludoviciana (Nutt.) S. Wats. *Proc. Am. Acad.* **23**: 252. 1888.

Myagrum argenteum Pursh, *Fl. Am. Sept.* 434. 1814. Not *Lesquerella argentea* S. Wats. 1888.
Alyssum ludovicianum Nutt. *Gen.* **2**: 63. 1818.

"Alyssum Spec. Bradbury."

Pursh does not say whose specimens he saw; they were gathered "on the banks of the Missouri."

(K) "1100 [miles] up the Missouri." This would be in South Dakota. The *Travels*, p. 337, says simply "on limestone rocks, Missouri."

41. (L) Sarracenia flava L. *Sp. Pl.* 510. 1753.

In an unknown hand: "Sarracinea flava *Pursh*, North America. 19 August 1809."

Presumably from the southeastern states, and perhaps not even collected by Bradbury, who left Virginia for St. Louis in August, 1809. There is a specimen from Pursh's herbarium, also without locality, in the Academy of Natural Sciences of Philadelphia.

42. (L) Ribes ? aureum Pursh, *Fl. Am. Sept.* 164. 1814.

"Ribes. Bradbury."

Pursh described his species from Lewis' specimens gathered "on the banks of the rivers Missouri and Columbia."

In the *Travels*, p. 336, from "Little Cedar Island, Missouri [South Dakota; see p. 78]."

43. (L) Potentilla strigosa Pall.; Pursh, *Fl. Am. Sept.* 356. 1814.

In an unidentified hand: "Potentilla lanata Mr. Bradbury."

(P) Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Potentilla pensylvanica β strigosa." Pursh's species, however, was based on a specimen by Lewis.

44. (P) Potentilla arguta Pursh, *Fl. Am. Sept.* 736. 1814.

Annotated by Lambert: "Louisiana, Bradbury" and by Pursh: "Potentilla arguta." TYPE.

(L) ISOTYPE.

Annotated by Shepherd: "Potentilla Pensylvanica Willd. Sp. Pl. P. 1094. Habitat in Louisiana (Bradbury)."

In the *Travels*, p. 337, this species is listed from the bluffs above the Aricara village, in South Dakota. This may be taken as the type locality.

45. (L) Prunus sp.

In an unidentified hand: "Cerasus. Prunus Chicasa? Mr. Bradbury."

Perhaps the "cherry" mentioned in letter no. 19.

46. (L) Sieversia triflora (Pursh) Richards. in Frankl. *Journey* ed. 2. 749. 1824.

Geum triflorum Pursh, *Fl. Am. Sept.* 736. 1814.

ISOTYPE? Annotated by Shepherd: "Geum triflorum Pursh flora Americana Vol. 2. Habitat in Upper Louisiana (Bradbury)."

(K) ISOTYPE ? "Prairies"; and in the *Travels*, p. 336, from the "head waters of Blackbird creek."

(P) TYPE ? The specimen is labeled "Geum triflorum" in Pursh's hand, but lacks other data.

This species seems imperfectly differentiated from *S. ciliata* (Pursh) G. Don. The specimen at Liverpool has the basal leaves hispid-ciliate as in the latter species, but not deeply dissected, merely toothed as in *S. triflora*. The specimen at Kew has no basal leaves.

47. Amorpha canescens Pursh, *Fl. Am. Sept.* 467. 1814.

Annotated in Shepherd's hand: "Amorpha pubescens Pursh's Flora Americana P. 466. V. 2. Habitat in Carolina (Bradbury)." A second sheet has: "Amorpha canescens Pursh's Flora P. 467 Habitat in Banks of Missouri & Mississippi (Bradbury)." Bradbury lists it in the *Travels*, p. 337, "on the prairie four miles west of St. Louis." Pursh says "on the banks of the Missouri and Mississippi..... v. v." Bradbury mentions also *A. fruticosa* and *A. microphylla*, but no specimens of these were found.

48. (P) Astragalus tenellus Pursh, *Fl. Am. Sept.* 473. 1814.

Ervum multiflorum Pursh, *Fl. Am. Sept.* 739. 1814.

Astragalus multiflorus A. Gray, *Proc. Am. Acad.* **6**: 226. 1864.

Annotated by Lambert: "[L]ouisiana. Bradbury" and by Pursh: "Ervum multiflorum"; also by Rydberg: "Homalobus tenellus (Pursh) Britton type." This represents the "fine specimens in the collection of Mr. Bradbury" mentioned by Pursh as the basis of *Ervum multiflorum*. The type of *Astragalus tenellus*, however, was a specimen by Lewis, "on the banks of the Missouri." Collected by Bradbury "opposite the Sepulchre bluffs, Missouri [Nebraska]" (*Travels*, p. 337).

(L) "Astragalus sulcatus ? Bradbury."

49. (L) *Astragalus missouriensis* Nutt. *Gen.* **2**: 99. 1818.

In Shepherd's hand: "Astragalus. J. Bradbury."

(P) Annotated in pencil: "Louisiana, Bradbury & Nuttall. Pursh's spec."

50. (P) *Astragalus triphyllus* Pursh, *Fl. Am. Sept.* 740. 1814.

Annotated by Lambert: "[Lou]isiana Bradbury" and by Pursh: "Astragalus triphyllus." TYPE.

51. (L) *Astragalus lotiflorus* Hook. *Fl. Bor. Am.* **1**: 152. 1831.

"I suppose an Astragalus Stem procumbent near the mouth of the Platte [Nebraska] with Yucca."

52. (P) *Astragalus racemosus* Pursh, *Fl. Am. Sept.* 740. 1814.

Annotated by Lambert: "Louisiana Bradbury" and by Pursh: "Astragalus racemosus." TYPE.

(L) ISOTYPE. This and nos. 50 and 53 are listed in the *Travels*, p. 337, "on the bluffs opposite the mouth of Papillon Creek [Nebraska], and at the Aricara villages [South Dakota]."

53. (P) Astragalus carnosus Pursh, *Fl. Am. Sept.* 740. 1814.

Annotated by Lambert: "[Lou]isiana. Bradbury" and by Pursh: "Astragalus carnosus." TYPE.

According to de Candolle (Prodr. **2**: 287), Pursh confused, under this name, a plant of *Sophora* with a fruit of *Astragalus*.

54. (L) *Astragalus ? plattensis* Nutt.; T. & G. *Fl. N. Am.* **1**: 332. 1838.

55. (L) *Astragalus trichocalyx* Nutt.; T. & G. *Fl. N. Am.* **1**: 332. 1838.

"Legume globular, fleshy, many seeded—cannot reduce it to any genus described. No. 103." The specimen, however, lacks the fruit.

56. (P) Astragalus longifolius (Pursh) Rydb. *Fl. Nebr.* **21**: 47. 1895.

Psoralea longifolia Pursh, *Fl. Am. Sept.* 741. 1814.

Annotated in pencil: "Psoralea longifolia Pursh A. pictus. Louisiana, Bradbury. Pursh's specimen." TYPE. Bradbury wrote in the *Travels*, p. 338: "near the Sepulchre bluffs [see no. 37]. *Probably not a Psoralea.*"

57. (L) Astragalus sp.

"Astragalus *spec.* Bradbury."

58. (P) Dalea aurea Nutt. *Fraser's Cat.* (nom. nud.). 1813; Pursh, *Fl. Am. Sept.* 740. 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Dalea aurea." This specimen is the basis of Pursh's description; but, since he ascribed the name to Nuttall ("*Fraser. Catal.* 1813.") the type must have been the plant grown by the Frasers and named by Nuttall. Bradbury found it "on the prairies six miles below the L'eau qui Court [Nebraska]" (*Travels*, p. 337).

59. (P) Dalea enneandra Nutt. *Fraser's Cat.* 1813. *Dalea laxiflora* Pursh, *Fl. Am. Sept.* 741. 1814.

Annotated by Lambert: "[L]ouisiana. Bradbury" and by Pursh: "Dalea laxiflora." TYPE of the latter name. There may be some question whether Nuttall's name is adequately characterized; if not, Pursh's name and type will be correct.

According to the *Travels*, p. 337, the species was collected at the Aricara village (in South Dakota).

60. (L) Glycyrrhiza lepidota Nutt. *Gen.* **2**: 106. 1818.

Liquiritia lepidota Nutt. *Fraser's Cat.* (nom. nud.). 1813.

"I cannot tell what genus to consider this plant as belonging to as I find [? no] roots. You will determine it.—Leguminosae. (Bradbury). No. 92."

Nuttall (Gen. **2**: 106) wrote: "Abundant around St. Louis, where it was first detected by Mr. John Bradbury, F. L. S.; it is also common on the alluvial banks of the Missouri to the Mountains . . ."; and in Fraser's Catalogue: "Met with from the source to the confluence of the Missourie. . . ."

61. (L) Medicago hispida Gaertn. *Fruct.* **2**: 349. 1791.

"Medicago—*spec.* Mr. Bradbury."

62. (P) Oxytropis lambertii Pursh, *Fl. Am. Sept.* 740. 1814.

Annotated by Lambert: "[Lou]isiana. Bradbury" and by Pursh: "Oxytropis argentata. Head of Clark's river [Montana]. July, 1806."

This is a mixture. According to Barneby, the part labeled A is *O. besseyi* Rydb. (*O. argentata* Pursh); B is *O. Lambertii* Pursh. Obviously Pursh's annotation refers to A (*cf. Fl. Am. Sept.*

473), Lambert's to B (*Fl. Am. Sept.* 740). B is therefore the TYPE of O. lambertii.

In the *Handbook and guide to the Herbarium Collections in the Public Museums, Liverpool,* p. 43, a specimen of this species is mentioned, collected by Bradbury "on the bluffs from the Maha village to the Poncars [Nebraska]." This was not found among the specimens I studied. The location given in the *Travels,* p. 338, duplicates that on the missing label.

63. (L) Petalostemum purpureus (Vent.) Rydb. *Mem. N. Y. Bot. Gard.* **1**: 238. 1900.

Dalea purpurea Vent. *Descr. Pl. Cels. pl. 40.* 1801. *Petalostemum violaceum* Michx. *Fl. Bor.-Am.* **2**: 50. 1803.

64. (P) Psoralea cuspidata Pursh, *Fl. Am. Sept.* 741. 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Psoralea cuspidata." TYPE. According to the *Travels,* p. 338, "on the bluffs near Chienne river [South Dakota]."

65. (P) Psoralea elliptica Pursh, *Fl. Am. Sept.* 741. 1814.

Annotated by Lambert: "[Lo]uisiana, Bradbury" and by Pursh: "Psoralea elliptica." TYPE. According to the *Travels,* p. 338, this species was collected from "sand hills near the Big Bend [South Dakota]."

Bradbury mentions in the *Travels,* in addition to the above species, *Psoralea esculenta* and *P. tenuiflora.*

66. (L) Strophostyles ? missouriensis (S. Wats.) Small, *Fl. SE. U. S.* 654. 1903.

Strophostyles angulosa var. *missouriensis* S. Wats.; A. Gray *Man.* ed. 6. 145. 1890.

"Phaseolus or is it a Galactia of Mich I can find no discription which will answer for this plant must therefore consider it as a—New Species Bradbury."

67. (P) Thermopis rhombifolia (Pursh) Nutt. *Gen.* **1**: 282. 1818.

Cytisus rhombifolius Pursh, *Fl. Am. Sept.* 741. 1814.

Annotated "137. Thermopsis rhombifolia." TYPE. And in pencil: "Louisiana. Bradbury. Pursh's spec!"

Two of the plants are in fruit, one in flower; in view of Pursh's remarks quoted below, this indicates that the collection is a mixture, only the fruiting specimen to be referred to the type.

(L) ISOTYPE. "Cytisus. Bradbury."

Pursh (Fl. Am. Sept. 742) wrote: "The specimen I have seen was in fruit, and Mr. Bradbury has seen only one plant in flower." This specimen is in fruit.

The locality mentioned in the *Travels,* p. 338, is: "at the mouth of the Chienne river, and on arid places near the Aricara village [South Dakota]."

68. (L) ? Trifolium sp.

"Trifolium. Mr. Bradbury."

69. (L) Vicia sparsifolia Nutt.; T. & G. *Fl. N. Am.* **1**: 270. 1838.

"Is probably a Vicia. Near the Otto Village on the Elk Horn a water of the Platte River [Nebraska]." "Orobus? Bradbury." "Orobus spec. Mr. Bradbury."

70. (P) Vicia americana Muhl.; Willd. *Sp. Pl.* **3**: 1096. 1803.

Annotated by Lambert: "[L]ouisiana. Bradbury" and by Pursh: "Vicia americana Willd. Sp. pl."

71. (P) Lathyrus stipulaceus (Pursh) Butters & St. John, *Rhodora* **19**: 163. 1917.

Vicia stipulacea Pursh, *Fl. Am. Sept.* 739. 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Vicia stipulacea." TYPE. Mentioned by Bradbury in the *Travels,* p. 337.

72. (L) Linum sp.

"Linum. Leaflets of the calyx lanceolate nerved one shorter than the next, leaves alternate . . . linear. Flowers yellow. Linum virginica. Bradbury. No. 88." Two species of *Linum* are mentioned in the *Travels,* p. 336: *L. lewisii,* "on Cannon Ball river"; and *L. rigidum,* "on the Missouri bluffs, common."

73. (L) Polygala viridescens L. *Sp. Pl.* 705. 1753.

"Polygala sanguinea ? Mr. Bradbury."

74. (L) Polygala alba Nutt. *Gen.* **2**: 87. 1818.

"Polygala. Mr. Bradbury."

75. (L) Croton monanthogynus Michx. *Fl. Bor.-Am.* **2**: 215. 1803.

"Plant shrubby semi-herbaceous covered with peltate hairs the branches and veins of the leaves with small brown glands—leaves oblong—heart-shaped. Flowers disposed in small terminal spikes—Hab. Land on Sullivans Island near Charles Town South Carolina (Elaeagnus ? Is a Croton: No. 93. Bradbury."

76. (L) Rhamnus lanceolata Pursh, *Fl. Am. Sept.* 166. 1814.

"Probably an Ilex. Islands of La R. Platte [Nebraska]. Prinos. Bradbury."

77. (L) Vitis sp.

In Shepherd's hand: "Vitis riparia Michaux, riparia Bradbury. Habitat in Louisiana."

78. (L) Ampelopsis cordata Michx. *Fl. Bor.-Am.* **1**: 159. 1803.

"Leaves slightly pubescent beneath. Racemes dichotomous one of the parts again subdivided. Flowers pentandrous and Polygamous probably an *Am-*

pelopsis but at present I am not in possession of a discription of that Genus. Ampelopsis Cordata ?"

79. (L) Hibiscus incanus Wendl. *Bot. Beob.* 54. 1798.

In Shepherd's hand: "Hibiscus palustris ? (Bradbury) Habitat in Upper Louisiana."

Bradbury's label must have been lost, since this must have come from a locality in the eastern states. Bradbury lists *"Hebiscus Militaris"* and *H. Manihot* from "American Bottom, Illinois" (*Travels*, p. 337).

80. (L)Sphaeralcea coccinea (Pursh) Rydb. *Bull. Torrey Club* **40**: 43–74. 1913.

Cristaria coccinea Pursh, *Fl. Am. Sept.* 453. 1814.

"Cristaria coccinea Bradbury." In Shepherd's hand: "Malva coccinea Habitat in Upper Louisiana, Bradbury."

(K) "L'eau qui court. Bradbury."

In the *Travels*, p. 337, this is listed "on the bluffs of the Missouri, above the L'eau qui court [Nebraska]." Pursh's species was founded on a collection by Lewis.

81. (L) Hypericum sp.

Annotated by Shepherd: "Hypericum galeoides Willd. Sp. Pl. P. 1451. Habitat in Carolina (Bradbury)."

82. (L) Elaeagnus commutata Bernh. *Allg. Thür. Gart.* **2**: 137. 1843.

Elaeagrus argentea Pursh, *Fl. Am. Sept.* 114. 1814. Not *Elaeagnus argentea* Moench, 1794.

(K) "Missouri."

In the *Travels*, p. 335, listed from "bluffs near the Mandan nation." See below under no. 83.

83. (L) Shepherdia argentea (Pursh) Nutt. *Gen.* **2**: 240. 1818.

Hippophaë argentea Pursh, *Fl. Am. Sept.* 115. 1814.

"This shrub is in Dioecia Octandria in which order I find nothing like it discribed. It was much past the time of florescence when I first found it and the male flowers were dropt off after some hours search I found a few broken branches with the male flowers unexpanded which enabled me to arrange it. The French who trade up the Missouri call it Grasse du Boeuf and say the berries are superior to cranberries." See the end of letter no. 18 and the beginning of no. 19. In the *Travels*, p. 335, the locality is stated as "Mahas, Platte, Ottoes, Missouri."

This and the preceding species were found mixed together; Bradbury's notes evidently apply to *Shepherdia;* there is no label for the *Elaeagnus*. Since Bradbury could distinguish the two (*Travels*, p. 335), and since they were collected hundreds of miles apart, the mixture probably occurred later, after they were received in Liverpool.

Both species were described by Pursh from collections on the Missouri by Nuttall and by Lewis. The type of *Hippophaë argentea* Pursh is in the Academy of Natural Sciences of Philadelphia. Nuttall created the genus *Shepherdia* from his own collections—named, of course, for John Shepherd of Liverpool.

84. (P) Gaura coccinea Pursh, *Fl. Am. Sept.* 733. 1814.

Annotated by Lambert: "Louisiana. Bradbury." TYPE.

As I pointed out in *Kew Bull.* **1934**: 57, *G. coccinea* Pursh and *G. coccinea* Nutt. are distinct though closely related taxa. However, the first publication of Nuttall's name was in Fraser's Catalogue of 1813 and is invalidated by the lack of description; *G. coccinea* Pursh (*G. parvifolia* Torr.) therefore takes precedence over *G. coccinea* Nutt. Gen. **1**: 249 (1818), which should probably be assigned infraspecific status.

The locality given in the *Travels*, p. 336, is "bluffs Aricara village."

85. (P) Oenothera albicaulis Pursh, *Fl. Am. Sept.* 733. 1814.

Annotated in pencil: "Louisiana, Bradbury. Pursh's spec." Both varieties or species discussed in *Kew Bull.* **1934**: 57 under the names *Anogra albicaulis* and *A. confusa* are here represented. The lefthand specimen is presumably the TYPE of *Oenothera albicaulis* Pursh.

(L) ISOTYPE. "Oenothera. N. Am.—Bradbury."

(K) ISOTYPE. "Little Cedar island [South Dakota]." This species is listed with the preceding in the *Travels*, p. 336, from "bluffs Aricara village."

86. (P) Erigenia bulbosa (Michx.) Nutt. *Gen.* **1**: 188. 1818.

Sison bulbosum Michx. *Fl. Bor.-Am.* **1**: 169. 1803.

Hydrocotyle composita Pursh, *Fl. Am. Sept.* 190. 1814.

Hydrocotyle ambigua Pursh, *Fl. Am. Sept.* 732. 1814.

Nuttall, having this specimen or a duplicate of it before him, identified Pursh's two supposed species, of which Pursh himself identified one with *Sison bulbosum* Michx. There is every likelihood, then, that this specimen, annotated by Lambert: "Louisiana. Bradbury," is the TYPE of *Hydrocotyle ambigua* Pursh, based on a Bradbury specimen. In the *Travels*, p. 336, this is listed from "rocks on the Mississippi, near Herculaneum."

87. (K) Cymopterus acaulis (Pursh) Rydb. *Bot. Surv. Neb.* **3**: 38. 1894.

Selinum acaule Pursh, *Fl. Am. Sept.* 733. 1814.

"100 up the Missouri." In the *Travels*, p. 336, "on the alluvion of the Missouri, from the river Naduet

to the Mahas." In the absence of a specimen annotated by Pursh, this may be designated as LECTOTYPE.

88. (L) Lomatium sp. near L. foeniculaceum (Nutt.) Coult. & Rose, *Contr. U. S. Nat. Herb.* **7**: 222. 1900.

"Altho this plant has a partial involucrum I think from that being one leaved & the Petals inflected it will come under the Genus *Apium*. I have only met with these two specimens on the Hills near Naduet [Kansas or Nebraska]."

"Gen. involucre none. Partial, one leaf to each umbellet. Root bulbous. Genus uncertain. Woody bottoms Missouri."

This differs from *Lomatium foeniculaceum* Coult. & Rose in that the inflorescence is not villous.

89. (K) Musineon divaricatum (Pursh) Coult. & Rose, *Bot. Gaz.* **20**: 259. 1895.

Seseli divaricatum Pursh, *Fl. Am. Sept.* 732. 1814.

"Bluffs of the Missouri." In the *Travels*, p. 336, "Missouri Bluffs, at the mouth of the L'eau qui court [Nebraska]." In the absence of a specimen annotated by Pursh, this may be designated LECTOTYPE.

90. (L) Gentiana sp.

According to Shepherd's label, which however is unreliable, this was from Pennsylvania. He identified it as *Gentiana quinqueflora* Willd.

91. (L) Phacelia purshii Buckl. *Am. Jour. Sci.* **45**: 171. 1843.

"Phacelia fimbriata ? but the style is long & bifid as in Hydrophyllum flower dilute blue. No. 110." Shepherd's label reads: "Phacelia fimbriata Persoon's Sinopsis p. 168, fimbriata Bradbury. Habitat in America."

According to the *Travels*, p. 336: "at Point L'Abbadie, on the Missouri, *with white flowers.*"

92. (L) Phlox pilosa L. *Sp. Pl.* 152. 1753.

"Phlox. leaves lance linear edges revolute. Plant hairy. Species nov ?"

93. (P) Cryptantha bradburiana Payson, *Ann. Mo. Bot. Gard.* **14**: 307. 1927.

Cynoglossum glomeratum Pursh, *Fl. Am. Sept.* 729. 1814. Not *Cryptantha glomerata* Lehm. 1836.

Annotated by Lambert: "[Lou]isiana. Bradbury" and by Pursh: "Cynoglossum glomeratum."

There are two specimens on the sheet, the right-hand one labeled in pencil: "Nuttall's specimen." If this is correct, the left-hand specimen is presumably the TYPE.

(L) ISOTYPE. "Anchusa ? Echium ? Bradbury."

(K) ISOTYPE.

In the *Travels*, p. 336, listed from the "Big Bend, Missouri [South Dakota]."

94. (L) Lithospermum canescens (Michx.) Lehm. *Pl. Asperif.* 305. 1818.

Batschia canescens Michx. *Fl. Bor.-Am.* **1**: 130. 1803.

"This seems most allied to *Lithospermum* but from the form of the Corol will probably form a new Genus—Root producing a most beautiful colour."

Bradbury was much impressed by the yellow puccoon. It is mentioned in letter no. 7, and in the *Travels*, p. 336 ("prairie about St. Louis"). The specimen very likely came from eastern Missouri.

95. (L) Lithospermum incisum Lehm. *Pl. Asperif.* 303. 1818.

Batschia longiflora Pursh, *Fl. Am. Sept.* 132. 1814. Not *Lithospermum longiflorum* Salisb. *Prodr.* 113. 1796.

"This plant is certainly a congener with no. 29 of the specimens of 1810 of which genus I have also seen a third species. I had considered it as a Lithospermum but am now inclined to doubt it—Tube very long segments of the petal fringed or undulate at the edges. Bradbury."

In the *Travels*, p. 336, "first occurs near the mouth of the Platte, on ascending the Missouri." It is now found throughout the Missouri valley as far east as the Mississippi.

The specimen is illustrated in the Liverpool *Handbook and Guide*, pl. 8. Pursh based his species on Nuttall's collection "on the banks of the Missouri," very likely gathered in Bradbury's company and possibly at the same time as this specimen.

The "third species" mentioned by Bradbury must be the *Lithospermum gmelini* (Michx.) Hitchc. listed in the *Travels*, p. 336, but not represented by specimens.

(K) "le haut Missouri."

96. (P) Mertensia lanceolata (Pursh) DC. *Prodr.* **10**: 88. 1846.

Pulmonaria lanceolata Pursh, *Fl. Am. Sept.* 729. 1814.

Annotated by Lambert: "Louisiana Bradbury" and by Pursh: "Pulmonaria lanceolata." TYPE.

Listed in the *Travels*, p. 335, as "opposite the Aricara village [South Dakota]."

97. (L) Verbena hastata L. *Sp. Pl.* 20. 1753.

"Verbena. Stamina 4. Leaves lanceolate acute Serrate opposite, petioled rough Stem square sides fluted spikes slender terminal. Is it hastata of Michaux? if so badly named. I would call it Prealta. No. 101."

98. (L) Physalis ? heterophylla Nees, *Linnaea* **6**: 463. 1831.

In an unidentified hand: "Physalis Pensylvanica LEG Augt. 15 1815 *Bradbury plant.*"

99. (L) Dasystemon sp.

"Gerardia. Bradbury."

100. (L) Castilleia sp.

"It agrees with the description of Bartsia Pallida but is either that or a non dis."

In the *Travels*, p. 337, Bradbury lists *Castilleia sessiliflora* from "Upper Louisiana."

101. (L) Castilleia coccinea (L.) Spreng. *Syst.* **2**: 775. 1825.

Bartsia coccinea L. *Sp. Pl.* 602. 1753.

102. (L) Pedicularis canadensis L. *Mant.* 86. 1767.

"Pedicularis canadensis ? Flowers *always* yellow Walter in Flor. Carol. says of this plant flor. purpur. —add to the discription *Flowers turning from right to left.* No. 86."

Shepherd's label reads: "Pedicularis canadensis Willd. Sp. Pl. P. 211. Habitat in South America (Bradbury)."

103. (P) Penstemon eriantherus Pursh, *Fl. Am. Sept.* 737. 1814.

In Lambert's hand: "1. Louisiana. Bradbury. 2. Hort. Boynton Mr. Nuttall." "Mr. Nuttall informs me this is Pen: cristatum Fraser's Catal: of Nuttalls plants not erianthera of the . . . specimens. Pentstemon erianthera Fl. Amer. Pursh." TYPE.

(K) "Sepulchre Bluffs [Nebraska]."

This specimen was identified as var. *saliens* (Rydb.) Pennell, *Contr. U. S. Nat. Herb.* **20**: 342 (1920) (see *Kew Bull.* **1934**: 60).

(L) This specimen conforms with the type, and may be listed as an ISOTYPE.

In Shepherd's hand: "Pentstemon erianthera Pursh Flora N. America 737. Habitat in Upper Louisiana. Bradbury."

The statement in the *Travels*, p. 337, makes it impossible to fix the type locality: "common on the bluffs from the Big Bend to the Aricara village [South Dakota]."

104. (P) Penstemon glaber Pursh, *Fl. Am. Sept.* 738. 1814.

Penstemon eriantherus Nutt. *Fraser's Cat.* (nom. nud.). 1813. Not *Penstemon eriantherus* Pursh, 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Penstemon glaber." TYPE.

(K) "Between Aricara and Mandan nation [North Dakota]." ISOTYPE.

In the *Travels*, p. 337, listed from "alluvion of the Missouri, above the Big Bend [South Dakota]."

105. (P) Penstemon angustifolius Pursh, *Fl. Am. Sept.* 738. 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Pentstemon angustifolium." In Bradbury's hand: "Pentstemon celestinum. high [?] hils. Mandans." TYPE.

In the *Travels*, p. 337, "near the Minetaree village [North Dakota]."

106. (P) Penstemon bradburii Pursh, *Fl. Am. Sept.* 738. 1814.

Penstemon grandiflorum Nutt. *Fraser's Cat.* (nom. nud.). 1813; *Gen.* **2**: 53. 1818.

Annotated by Lambert: "Penstemon Bradburii Fl: Am: Sep:" TYPE.

(K) "Chienne rivière [South Dakota]." ISOTYPE.

Opinion may perhaps differ on whether the brief statement in Fraser's *Catalogue* constitutes a sufficient description to make publication of Nuttall's name valid in 1813. Here it is: "Flower about the size and form of *Digitalis purpurea*, and spontaneously subject to great variation in color." If this be considered plain enough, then of course Nuttall's name will take precedence over Pursh's, and these specimens will not be type material of the name.

107. (P) Penstemon gracilis Nutt. *Gen.* **2**: 52. 1818.

The specimen is labeled by Lambert: "Louisiana, Nuttall"; and by Pennell: "collected by Bradbury." If the latter statement is correct, this is the TYPE.

(K) ? ISOTYPE. "Prairies." The locality mentioned by Nuttall ("From the Arikarees to Fort Mandan . . .") makes it probable that the specimen was collected on Bradbury's overland journey from the Aricara village to Fort Mandan.

(L) ? ISOTYPE. Annotated by Shepherd: "Pentstemon (Bradbury) Habitat in Upper Louisiana."

108. (L) Ruellia strepens L. *Sp. Pd.* 634. 1753.

"Didynamia Angios.—Probably a Ruellia but I cannot reconcile it to any discribed species *affine* or *Strepens.*"

109. (L) Specularia perfoliata (L.) A.DC. *Monog. Camp.* 351. 1830.

Campanula perfoliata L. Sp. Pl. 169. 1753.

"Campanula seems to agree with Campanula perfoliata excepting that *Walter* says of C. P. *floribus sessilibus aggregatis*—In the Priaries about St. Louis. May [1810]. No. 111." "Amplexicaulis of Michaux."

110. (P) Plantago elongata Pursh, *Fl. Am. Sept.* 729. 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Plantago elongata." TYPE.

Also "Type of *Plantago elongata* Pursh Fide E. L. M. [Edward Lyman Morris] Feb. 13, '09."

According to the *Travels*, p. 335, from "near the Maha village [Nebraska]."

(K) "Sepulchre Bluffs [Nebraska]." ISOTYPE.

111. (P) Plantago purshii R. & S. *Syst. Veg.* **3**: 120. 1818.

Plantago lagopus Pursh, *Fl. Am. Sept.* 99. 1814. Not *Plantago lagopus* L. 1753.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Plantago Lagopus." The species was based on Nuttall's collection on the Missouri.

(L) Annotated by Shepherd: "Plantago albicans Willd. Sp. Plant. P. 645. Bradbury Upper Louisiana."

In the *Travels,* p. 335, listed on the "Alluvion of the Missouri, common."

112. (L) Artemisia sp.

"Artemisia. Bradbury."

113. (L) Artemisia sp.

"This species of Artemisia I have only seen in its present state it is the fifth american species I have seen—Rivière Platte. Bradbury."

Four species of Artemisia are listed in the *Travels,* p. 338, as "common on the Missouri." One of these, *A. dracunculoides,* was described by Pursh (*Fl. Am. Sept.* 742) from a specimen by Bradbury. Unfortunately these specimens have not been preserved. The above is a mere fragment.

114. (L) Aster sericeus Vent. *Descr. Pl. Cels.* pl. 33. 1801.

Annotated by Shepherd: "Aster argentea (Bradbury) Habitat in North America."

The collector of the specimens in Lambert's herbarium seen by Pursh was not named by him. They were found "on the banks of rivers, among rocks, in the Illinois country, and on the Mississippi and Missouri" (*Fl. Am. Sept.* 548). Bradbury collected it on a "prairie behind St. Louis, abundant" (*Travels,* p. 338).

(P) Annotated by Lambert: "Upper Louisiana Nuttall" and by Pursh: "Aster argenteus. Pratis St. Louis. Rapids of Fox River.

115. (L) Bidens sp.

116. (L) Bidens sp.

These two were grown in the Liverpool Botanic Garden from seed sent by Bradbury in 1811.

117. (L) Chrysopis ? foliosa Nutt. *Trans. Amer. Philos. Soc.* II. **7**: 316. 1841.

"Inula (Bradbury)."

118. (L) Coreopsis sp.

"I have no hesitation in thinking this is a Coreopsis but there is no discription in Systema Natura that will answer for it, but it is so abundant that I am in great doubt when I call it Is it New Species."

119. (L) Erigeron ? pulchellus Michx. *Fl. Bor.-Am.* **2**: 134. 1803.

"Erigeron bellidifolium (Bradbury) Habitat in North America."

120. (L) ? Eupatorium sp.

"*Eupatorium.* Does not appear to be discribed in Sys. Nat. . . . Flowers cream colored odorous calyxes containing from 12 to 16 florets."

121. (L) Eupatorium altissimum L. *Sp. Pl.* 837. 1753.

"Eupatorium. Has partly the character of E. cuneifolium but does not wholly agree leaves serrate near the point, lance oblong. calyces with 5 florets. No. 99."

In the *Travels,* p. 338: "Missouri and Mississippi, common."

122. (P) Tetraneuris acaulis (Pursh) Greene, *Pittonia* **3**: 265. 1898.

Galardia acaulis Pursh, *Fl. Am. Sept.* 743. 1814.

Annotated by Lambert: "Louisiana. Bradbury" and by Pursh: "Galardia acaulis." TYPE.

In the *Travels,* p. 338, "on the Missouri near the Aricara village. *Probably a Chaptalia.*"

(L) A specimen labeled by Bradbury: "Inula. Bradbury" may belong with the above.

123. (L) Grindelia lanceolata Nutt. *Jour. Acad. Phila.* **7**: 73. 1834.

"I cannot at present determine this plant it seems to belong Syngenesia superflua or necessaria. Seeds with 2 or 3 long awns receptacle naked—On the first descent beyond the Sand Stone Rocks 2 miles from Sandy Creek on the Mine Road.

Palmer and Steyermark (*Ann. Mo. Bot. Gard.* **21**: 515. 1934) reported this species from Jefferson County above bluffs of St. Peter Sandstone—probably very near Bradbury's place of collection.

124. (L) Helianthus sp.

"*Helianthus.* Leaves opposite Petioled three nerved acutely lanced lower ones serrate upper entire upper side rough with appressed bristles Stem round smooth scales of the Calyx narrow loose. No. 62. not discribed in Sys. Nat."

125. (L) ? Helianthus sp.

"*Helianthus.* Does not appear to have been discribed—Stalk nearly naked Stem almost always three flowered. No. 81."

126. (P) Hymenopappus tenuifolius Pursh, *Fl. Am. Sept.* 742. 1814.

Annotated by Lambert: "Louisiana. Bradbury." TYPE.

127. (P) Lepachys columnaris (Pursh) T. & G. *Fl. N. Am.* **2**: 315. 1842.

Rudbeckia columnaris Pursh, *Fl. Am. Sept.* 575. 1814.

Annotated by Lambert: "Louisiana. Bradbury."

Bradbury wrote (*Travels*, p. 338): "bluffs above the Aricara village. *Most probably not a Rudbeckia, and ought to form a new genus.*"

128. (L) Rudbeckia hirta L. *Sp. Pl.* 907. 1753.

129. (P) Senecio plattensis Nutt. *Trans. Amer. Philos. Soc.* II. **7**: 413. 1841.

Two specimens, annotated by Lambert: "Upper Louisiana. Nuttall"; "Louisiana. Bradbury" and by Pursh: "Cineraria heterophylla."

This is perhaps the *Senecio panperculus* of the *Travels*, p. 338, collected on the "prairie below the L'eau qui Court."

130. (L) Solidago sp.

"Solidago. very like *procera* Bradbury. LBG Nov 2. 1815."

131. (P) Agoseris glauca (Pursh) Greene, *Pittonia* **2**: 176. 1891.

Troximon glaucum Pursh, *Fl. Am. Sept.* 505, 742. 1814.

132. (L) Vernonia ? missurica Raf. *Herb. Raf.* 28. 1833.

"*Vernonia* Species nova American Bottom Illinois Territory near the new town of Harrison. August [1810]." The specimen is in fruit, lacks leaves.

The following specimens are fragments not determined by me.

133. (L) "This Potamogeton is certainly not discribed in the Sys. Nat. nor indeed any American one—Leaves oval seven nerved opposite."

134. (L) "Pentandria Monogynia. *Calyx.* Five leaved, leaves eggd [?] concave—*Corol.* Small, campanulate, border ten cleft five of the divisions three parted. Middle segment concave inclosing the stamen. lateral segments & the other five divisions of the corol bending over the style. Stamens. Inserted on & shorter than the corol. Pistiln. Stigma simple style short Geny. becoming a one seeded berry. Shrub 20 feet high. Leaves oval entire glabrous. Flowers small in clusters."

135. (L) "*Arethusa pendula* of Michaux. No. 130."

136. (L) "Cheiranthus spec. Mr. Bradbury." Not a crucifer; the label evidently misplaced. *cf. Kew Bull.* **1934**: 55, *Erysimum asperum.*

137. (L) In Shepherd's hand: "Lysimachia linearis Bradbury Habitat in America."

138. (L) "Amorpha ? spec. Bradbury." Not an Amorpha.

139. (L) A mixture of three woody species. "This will certainly form a new genus. No. 129."

APPENDIX I

The following is a list of the 44 species described by Pursh in his *Supplementum* from specimens collected by Bradbury. Of these 36 are accounted for by specimens enumerated in the foregoing list; they are indicated below by asterisks (*).

p. 728. *Aristida pallens.
*Stipa membranacea.
Cynosurus secundus.
*Allionia linearis.
*Allionia hirsuta.
p. 729. *Plantago elongata.
*Pulmonaria lanceolata.
*Cynoglossum glomeratum.
p. 732. *Hydrocotyle ambigua.
*Selinum acaule.
*Seseli divaricatum.
p. 733. *Rumex venosus.
*Gaura coccinea.
*Oenothera albicaulis.
p. 735. Eriogonum pauciflorum.
p. 736. *Geum triflorum.
*Potentilla arguta.
*Ranunculus multifidus.
p. 737. *Pentstemon erianthera.
p. 738. *Pentstemon angustifolia.
*Pentstemon glabra.
Castilleia sessiliflora.
p. 739. Cleome pinnata.
*Vicia stipulacea.
*Ervum multiflorum.
p. 740. *Astragalus racemosus.
*Astragalus carnosus.
*Astragalus triphyllus.
*Oxytropis lambertii.
*Dalea aurea.
p. 741. *Dalea laxiflora.
*Psoralea elliptica.
*Psoralea longifolia.
*Psoralea cuspidata.
*Cytisus rhombifolius.
p. 742. Troximum cuspidatum.
*Hymenopappus tenuifolius.
Artemisia dracunculoides.
Erigeron hirsutum.
p. 743. *Galardia acaulis.
Iva axillaris.

APPENDIX II

The following list contains the names included in Bradbury's "Catalogue of some of the more rare or valuable plants discovered in the neighbourhood of St. Louis and on the Missouri" (pp. 335–338 of *Travels in the interior of America, in the years 1809, 1810, and 1811*). Those that are represented by specimens mentioned in the present paper are marked with asterisks (*).

p. 335. Leersia lenticularis.
*Aristida pallens.
Stipa juncea.
*Stipa membranacea.

Aira brevifolia.
Festuca spicata.
Cynosurus secundus.
Hordeum jubatum.
*Allionia ovata.
*Allionia linearis.
*Allionia hirsuta.
*Plantago lagopus.
*Plantago elongata.
*Elaeagnus argentea.
*Hippophaë argentea.
Pulmonaria sibirica.
*Pulmonaria lanceolata.
p. 336. *Batschia canescens.
Batschia gmelini.
*Batschia longiflora.
Onosmodium molle.
Dodecatheon meadia.
*Phacelia fimbriata.
*Cynoglossum glomeratum.
Solanum heterandrum.
*Ribes aureum.
Salsola depressa.
*Hydrocotyle ambigua.
*Selinum acaule.
*Seseli divaricatum.
Linum lewisii.
Linum rigidum.
*Yucca angustifolia.
Lilium catesbaei.
Lilium umbellatum.
*Rumex venosus.
*Gaura coccinea.
*Oenothera albicaulis.
Oenothera macrocarpa.
Eriogonum pauciflorum.
Eriogonum sericeum.
Cactus viviparus.
Bartonia ornata.
Bartonia nuda.
*Geum triflorum.
p. 337. *Potentilla arguta.
*Ranunculus multifidus.
Stachys foeniculum.
Capraria multifida.
Martynia proboscidea.
*Penstemon erianthera.
*Penstemon angustifolia.
*Penstemon glabra.
Castilleia sessiliflora.
*Myagrum argenteum.
*Erysimum lanceolatum or Cheiranthus erysimoides.
Cleome pinnata.
*Cristaria coccinea.
Hebiscus militaris.
Hebiscus manihot.
*Ervum multiflorum.
*Vicia stipulacea.
Lathyrus decaphyllus.
Lupinus pusillus.
Amorpha fruticosa.
Amorpha microphylla.
*Amorpha canescens.
*Astragalus racemosus.
*Astragalus triphyllus.
*Astragalus carnosus.
*Dalea aurea.
*Dalea laxiflora.
p. 338. *Psoralea cuspidata.
*Psoralea longifolia.
*Psoralea elliptica.
Psoralea esculenta.
Psoralea tenuiflora.
*Cytisus rhombifolius.
Sonchus pulchellus.
Troximum cuspidatum.
*Eupatorium altissimum.
*Oxytropis lambertii.
Artemisia dracunculus.
Artemisia cana.
Artemisia campestris.
Artemisia santonica.
Arnica fulgens.
Cineraria integrifolia.
Erigeron hirsutum.
Erigeron divaricatum.
*Senecio pauperculus.
*Aster argenteus.
Amellus villosus.
Amellus spinulosus.
*Galardia acaulis.
*Rudbeckia columnaris.
Iva axillaris.
Cheilanthes dealbata.
*Cheilanthes vestita.